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ABSTRACT

In order to obtain information about intermediate educational outcomes, especially the post secondary experience, this followup study of the Upward Bound Program (UB) was conducted. The study focused on educational progress and persistence, but also investigated student's educational expectations and problems experienced in obtaining an education. The unique relationships between certain educational outcomes, student characteristics, program context, and the length of exposure to the UB program were also examined. Background information on the Upward Bound Program and the National Evaluation are provided, as well as an overview of the Base-Year Study and the Followup Study. It was found that the overall educational progress of former UB participants is greater than that of the nonparticipants, and that, within the UB group, progress is greater with longer program participation. Two or three years of UB participation were associated with an increase in expected educational progress significant enough to offset the decrease associated with being an academic risk. The effects of UB participation were essentially the same for all racial groups, males and females, poverty level and nonpoverty students, academic risks as well as nonrisks, tenth, eleventh, and twelth graders, and all project sites. Supporting technical matherials related to sampling, data collection and analysis, and copies of the student questionnaires are appended. (Author/MV)

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Final Report

EVALUATION STUDY OF THE UPWARD BOUND PROGRAM: A FIRST FOLLOW-UP

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by

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Prepared for

Office of Planning, Budgeting, and Evaluation
U.S. Office of Education

under
Contract No. OEC-0-73-7052

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PREFACE AND ACKNOWLEDGEMENTS

This document, Evaluation Study of the Upward Bound Program: A First Follow-Up, represents the final report of research conducted by the Research Triangle Institute (RTI) under U.S. Office of Education (USOE) contract number OEC-0-73-7052 (modification number 9). The main body of the report, an executive summary, and supporting technical appendixes are bound in one volume.

The authors, in preparing this report, wish to express their special appreciation for the cooperation and contributions of a number of people both within and outside RTI.

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Executive Summary

Evaluation Study of the Upward Bound Program: A First Follow-Up

I. BACKGROUND AND PURPOSE

Under authority of the Economic Opportunity Act of 1964, as amended (42 U.S.C. 2809), the Office of Economic Opportunity (OEO) funded 17 Upward Bound (UB) projects as a pilot program in the summer of 1965. In 1966, UB was authorized as a national program under Title II.A of the Economic Opportunity Act. On July 1, 1969, the UB program was transferred from OEO to the U.S. Office of Education (USOE), Department of Health, Education, and Welfare. The present legislative authority is the Education Amendments of 1976 (Public Law 94-482).

The program was designed to reach low-income high school students who have potential for successfully completing an educational program, but who, due to inadequate preparation and/or lack of motivation, are prevented from meeting conventional criteria for admission to a postsecondary program. Through the use of various intervention strategies (e.g., remedial instruction, exposure to new or altered curricula, tutoring, cultural enrichment activities, counseling, and encouragement) the program is legislatively mandated to generate in participants the skills and motivation necessary to enter and successfully complete postsecondary education (PSE).

During the summer, UB participants typically reside on a postsecondary campus for an intensive six- to eight-week session of courses, cultural and social events, and counseling. In the academic year, they typically receive less intensive attention (e.g., Saturday classes; periodic counseling). During their junior and senior years of high school, participants receive guidance in exploring options for postsecondary preparation.

In July 1973, USOE contracted with the Research Triangle Institute (RTI) to plan and conduct an evaluation of the UB program. The initial (base-year) study (ED 131-152 and 131-153) provided a comprehensive description of the UB program as it existed in program year 1973-74, investigated the relationships of UB participation to short-term educational outcomes, and examined the relative effectiveness of different types of UB projects in respect to these short-range outcomes; however, the base-year study provided no information regarding postsecondary progress and persistence.

The follow-up study reported herein was conducted between October 1976 and September 1977 to obtain information about intermediate educational outcomes, particularly as related to the postsecondary experience. One major focus of the follow-up study was educational progress and persistence as related to extent of prior participation in the UB program, but the study also allowed investigation of student educational expectations, problems experienced in obtaining an education, and other outcomes. Another major purpose was to examine the unique relationships between certain educational outcomes and student characteristics, temporal factors, and contextual factors, in addition to extent of UB participation, to gain a fuller understanding of potential program effectiveness. The study also provided a vehicle through which the results of the base-year study could be reevaluated.

II. DESIGN AND METHODOLOGY

The design of the follow-up study was necessarily constrained by the design of the preceding base-year study. From the 333 regular UB projects operating in the coterminous United States during the 1973-74 program year, 54 were selected after stratification on student ethnicity, number of students served, project location, type of project, and type of host institution. All participants in the sampled projects who were in grades 10, 11, or 12 were selected, yielding 3,710 UB participants in the final sample. An average of two high schools providing students to each selected UB project were selected, and, from a sample of classrooms in each of these schools, a total of 2,340 nonparticipating comparison students were selected after stratification on grade level, ethnicity, low-income status, and academic risk.

Follow-up data were collected during the winter and early spring of 1977 from members of the base-year study sample, through a mail survey and through telephone interviews of a subsample of nonrespondents to the mailing. Other data were obtained from existing sources: the base-year student master file, a postsecondary institutional data file, and USOE records. Overall response rate for the study was 71 percent; however, sample coverage was considerably higher (95 percent). The extent of indeterminate data was small; inconsistency within the data was generally within reasonable tolerance levels; and data reliability was generally acceptable.

Two conceptually different, but related, approaches to analysis were undertaken. The first approach was comparative-descriptive and addressed questions of national program impact by comparing UB participant outcomes with outcomes for a statistically equated group of nonparticipants. The second approach was relational; multiple regression was employed to predict educational outcomes on the basis of individual background factors, temporal and contextual factors, extent of program participation, and the various interactions of these factors. Sample weights were used in analysis, where appropriate and feasible, to account for unequal selection probabilities, and weight adjustment procedures were implemented so that nonrespondents would be represented in the final results by respondents who were most like them. Appropriate standard errors were computed for all comparative-descriptive results and, when feasible, for the relational results.

III. OVERVIEW OF MAJOR FINDINGS

The current findings and findings from the base-year are quite consistent in suggesting UB program impact on those outcomes where it would be most expected--educational expectations, progress, and persistence. Moreover, in respect to progress and persistence, the suggested program impact seems to be focused at the transition step between high school completion and postsecondary entry. There is no defensible indication of program effect on high school completion or on progress and persistence once postsecondary entry has been gained.

Although the increments in educational outcomes as a function of program participation appear to be of sufficient magnitude to offset some of the decrements associated with being over age-in-grade, poverty level, academic risk, or ethnic minority, these increments are not sufficient to offset all such decrements.

The suggested program impact is also typically incremental--greater impact with greater exposure to the program. The most clear-cut incremental relationship is associated with educational expectations, for which each added year of program exposure provides a well-defined increase in expectations. For the progress and persistence measures, however, suggested incremental impact appears to be an all-or-none function, with little or no increase as a result of one year's participation and with a more-or-less equivalent increase attributable to either two- or three-years' participation. This relationship of extent of program participation to educational progress and persistence is also seen to be focused in the area of postsecondary entry.

Difference in the patterns of educational progress for UB participants and nonparticipants is presented in Figure 1. The values given in that figure are composites from the tenth- and twelfth-grade cohort results and include: (1) conditional rates of advancing to a subsequent educational level, given attainment of a previous level (in parentheses) and (2) percentages of the original groups attaining each educational level (in brackets). The figure shows clearly that the greater overall progress of former program participants is primarily attributable to the greater rates at which they enter PSE.

The postsecondary entry rates should be considered, however, in light of differences in the types of institutions entered and the types of individuals who enter. Former program participants enter four-year institutions, institutions with more attractive financial aid policies, and institutions hosting UB projects in greater relative numbers than nonparticipant comparisons. Among the postsecondary-entering subgroup of nonparticipants, males, poverty status individuals, ethnic minorities, and academic risks are underrepresented. Such underrepresentation was not observed among former UB participants, suggesting that the program may be assisting participants in overcoming some long-standing barriers to PSE.

The specific examination of progress and persistence following PSE entry showed no advantage for former participants but was basically inconclusive. The results involve progress and persistence measures after a maximum of only three and one-half years following high school completion. It is unrealistic to expect that these measures are not deeply confounded with the individual's pace of educational advancement. Among the population of nontraditional students being considered, temporary interruptions in educational progress and

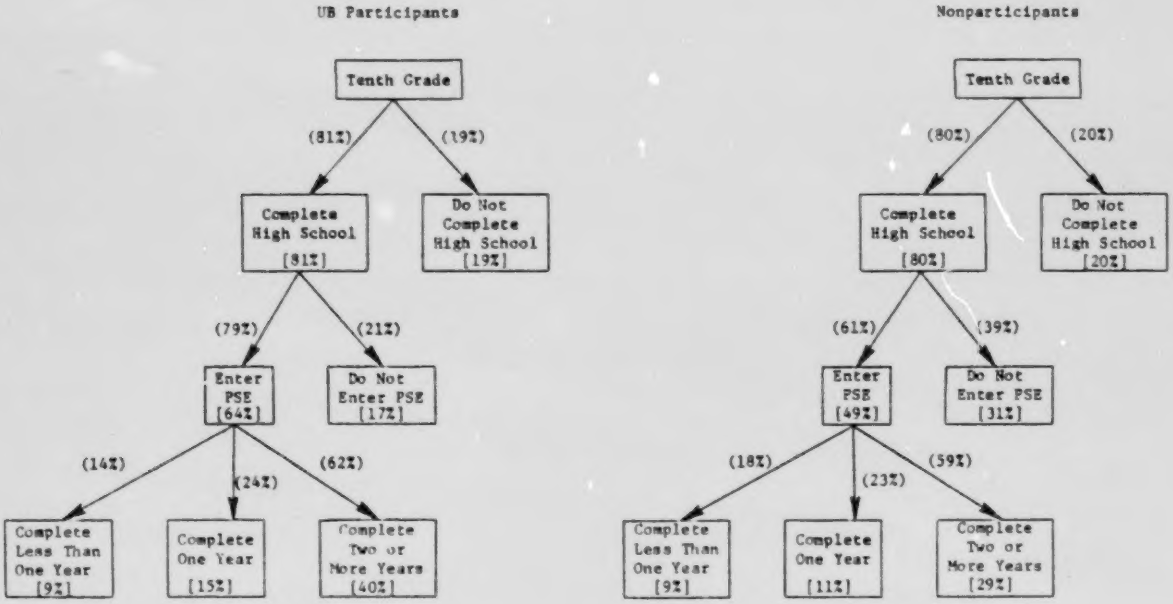
UB Participants
Tenth Grade
(81%)
(19%)
Complete High School [81%]
Do Not Complete High School [19%]
(79%)
(21%)
Enter PSE [64%]
Do Not Enter PSE [17%]
(14%)
(24%)
(62%)
Complete Less Than One Year [9%]
Complete One Year [15%]
Complete Two or More Years [40%]
Nonparticipants
Tenth Grade
(80%)
(20%)
Complete High School [80%]
Do Not Complete High School [20%]
(61%)
(39%)
Enter PSE [49%]
Do Not Enter PSE [31%]
(18%)
(23%)
(59%)
Complete Less Than One Year [9%]
Complete One Year [11%]
Complete Two or More Years [29%]

Figure 1. Composite of educational progress of Upward Bound participants and nonparticipants

persistence toward ultimate educational goals are to be expected. While no general impact of UB program participation on postsecondary success was suggested by the current results, this in no way suggests that impact would not be indicated by more long-term measures.

While it is impossible to state definitively that the greater expectations, progress, and persistence of former UB participants were brought about (or caused) by their exposure to the program, such a conclusion appears to be the most likely one. The pattern of results are closely alligned with what one might expect as a result of the common areas of project emphases and strategies, and the incremental nature of the outcomes for different lengths of program exposure is also quite intuitive in light of program operations.

Virtually all projects emphasize the importance of postsecondary entry and assist participants in applying for and gaining both admission to some postsecondary institution and financial aid from that institution. It would be surprising if these efforts were not reflected in greater entry rates of participants. One would also expect greater attendance by former participants in four-year institutions, UB host institutions, and institutions with attractive aid policies, given the implicit program placement emphases. The importance of the placement function of the program is further supported by findings that participants who were in the program during their senior year (the time during which postsecondary application and admission is most heavily stressed) enter postsecondary education at a greater rate than participants who were not.

Similarly, projects uniformly provide support and counseling to participants toward increasing self-confidence in their abilities and toward raising their educational sights. With these treatments and with the advantage realized in gaining postsecondary entry, the greater expectations of former participants are also not surprising.

There are, nonetheless, alternate plausible explanations for the findings, the most likely of which is that program participants initially enter the program partly because of their existing aspiration for education beyond high school. This possibility could not be investigated directly, since no measure of pre-program motivation was available; however, the base-year findings indicated that many projects selected participants, in part, on the basis of their expressed desire to enter PSE.

Whatever impact the UB program may have on the outcomes of participants, it appeared to be pervasive and egalitarian. There was no indication of differential project effectiveness on any of the outcome measures considered. Both of the major analyses suggested that any benefits derived from the program by participants were more-or-less the same regardless of the individual's sex, race, poverty status, or risk status.

IV. SUMMARY AND INTERPRETATION OF SPECIFIC FINDINGS

A. Educational Expectations

A greater percentage of former participants (55 percent) expected to complete at least a four-year college education than did nonparticipants (40 percent), and expectations were generally higher given longer participation in the UB program. This finding is similar to the results of the base-year study; however, while the current expectations of nonparticipants were slightly less than those reported during the base-year, the current expectations of participants were slightly higher. It was further suggested that a single year of program participation was associated with a significant increase in expectations that was sufficient to compensate for the decrease associated with being either poverty-level or over age-in-grade. The increase in expectations associated with three years of program participation effectively compensated for the combined decrease associated with being an academic risk and within poverty level.

B. Educational Problems Experienced

Greater proportions of former UB participants than nonparticipants reported having experienced problems in educational progress due to poor high school preparation, family finances, and inadequate financial aid. This finding is not particularly surprising when one considers that former participants have higher educational expectations, that they generally attend different kinds of postsecondary institutions, and that they may have been sensitized to these problems through UB program counseling.

C. High School Completion

In general, former UB participants, demonstrated no greater high school completion rates than nonparticipants. These results were in basic agreement with results of the base-year study. Secondary completion rates were high for both participants and nonparticipants, and the rates were greater for those in the higher grades initially, as might be expected. High school completion rates for UB participants were 81 percent, 91 percent, and 96 percent for the tenth-, eleventh-, and twelfth-grade cohorts, respectively. Analogous rates for nonparticipants were 80 percent, 86 percent, and 96 percent.

D. Postsecondary Entry Rates

Within each of the three grade cohorts, the overall PSE entry rate for former UB participants was greater, by almost 20 percentage points, than that for nonparticipants. Among nonparticipants who had completed high school, entry rates were 49 percent, 58 percent, and 61 percent for the tenth- through twelfth-grade cohorts, respectively. Analogous rates for former UB participants were 68 percent, 73 percent, and 79 percent. An examination of the incidence of delayed PSE entry indicated that substantial percentages of those individuals who had not entered postsecondary education during the base-year subsequently entered after a delay. Such delayed entry was greater for former participants (62 percent) than for nonparticipants (36 percent).

Also, within each of the three grade cohorts, longer participation in the UB program was associated with greater PSE entry rates. For the total participant group, PSE entry rates were 63 percent, 75 percent, and 82 percent, respectively, for those with one-, two-, or three-years' participation in the program. One year of program participation provided a negligible increase in PSE entry rate, but two or more years of program participation was associated with a significant increase in the likelihood of entry. Three years' exposure to the program compensated for the combined decrease in likelihood of postsecondary entry associated with being an academic risk and being one year over age-in-grade.

E. Types of Individuals Entering Postsecondary Education

Among UB participants, the proportions of ethnic minorities, males, poverty status individuals, and academic risks in the postsecondary-entering subgroup coincided with the analogous proportions in the total participant population. Among the PSE-entering subgroup of nonparticipants, however, all of these groups were underrepresented. This suggests that PSE entry is more egalitarian among former program participants than nonparticipants.

F. Types of Postsecondary Institutions Entered

A greater percentage of former UB participants (66 percent) reported attending four-year institutions or universities than did nonparticipants

(51 percent). Former participants also reported greater attendance at institutions hosting an Upward Bound project (47 percent) than nonparticipants (23 percent). Attendance at Special Services for Disadvantaged Students (SSDS) host institutions was slightly, but not significantly, greater among former participants, and UB participants attended institutions with more attractive financial aid policies at somewhat greater rates than nonparticipants. The finding regarding institutional level of offering is directionally consistent with results of the base-year study.

The considerably greater rate at which former participants attended UB host institutions cannot be explained by attendance proximity since the nonparticipant sample was drawn from the same locations as the participant sample. Host institutions are committed to the program and to serving its participants, and this finding indicates that enrollment of former participants in host institutions is disproportionately large relative both to the nonparticipant enrollment and to the proportion of host institutions in the institution population. It is suspected that at least part of this difference is attributable to the fact that a fairly large number of UB participants are admitted to the particular institution that served as host for the specific UB project in which they participated.

G. Postsecondary Persistence and Progress

Investigations related to this topic were restricted to the subsets of former UB participants and nonparticipants who entered some form of postsecondary schooling, and, from the findings already noted, it is obvious that these investigations could not have provided completely clear-cut results. The former participants entering PSE differed from entering nonparticipants, their educational expectations were greater, they entered different kinds of postsecondary institutions, and they reported having experienced more problems in fulfilling their educational aspirations.

No consistent or systematic difference was detected between former UB participants and nonparticipants in respect to postsecondary progress, and no differences were observed as a function of length of program participation. The one hint of a relationship between program participation and progress was for the eleventh-grade cohort, in which former participants held a marginally significant advantage.

Differences were observed in the current enrollment rates of former participants and nonparticipants; nonparticipants who had entered PSE were currently enrolled at somewhat higher rates (76 percent) than were former participants (67 percent), but no differences in current enrollment were observed as a function of length of program participation. This finding for the aggregate group was primarily attributable to the difference observed within the twelfth-grade cohort. In general, current enrollment rates for both participants and nonparticipants tended to decrease from the tenth- to the twelfth-grade cohort.

These findings indicate that while former UB program participants have progressed as far in postsecondary education as nonparticipants they are currently enrolled in smaller relative numbers. The fact that participants demonstrated slightly greater progress than nonparticipants in the eleventh-grade cohort and lower current enrollment than nonparticipants in the twelfth-grade cohort suggests that postsecondary attrition among former participants is greater than that of nonparticipants following the second year of PSE but that through the second year participants are progressing as well or better than nonparticipants.

No systematic or meaningful differences in educational progress as a function of institution type or of perceived adequacy of financial aid were uncovered; however, the disparity between current enrollment of former participants and nonparticipants was concentrated within the universities, two-year schools, UB host institutions, and SSDS host institutions, as well as among the subgroup of individuals reporting problems with adequacy of financial aid. These differential disparities suggest a complex pattern that resists an immediate or simple explanation, particularly in light of other findings.

H. Overall Educational Progress

A seprate examination of this more general progress measure indicated that the overall educational progress of former UB participants is greater than that of the nonparticipants and is greater with longer participation in the program. This finding held within each of the three grade cohorts considered. The increase in educational progress associated with one year of program participation was not significant, and the increase associated with two years' participation was only slightly less than that associated with three years' participation; either increase was sufficient to compensate almost completely for the decrease associated with being an academic risk. The basic nature of the difference in overall educational progress of UB participants and nonparticipants has been presented in Figure 1.

I. Additional Findings

One of the most surprising findings was that the relationships between educational expectations and progress and the variables used to predict them were quite stable regardless of the specific project site involved or the specific characteristics of the sample member. A particularly relevant implication from this finding is that any suggested impact on these intermediate educational outcomes attributable to UB participation is effectively the same for blacks, whites, or other ethnic groups; for males or females; for poverty level and nonpoverty individuals; and for academic risks as well as nonrisks.

The lack of an interactive relationship between project association and extent of participation in the program is of particular importance since it implies that differential project effectiveness can be discounted in explaining variation in the outcome measures selected. That is, when student characteristics, temporal factors, contextual factors, and length of program participation are accounted for, only negligible differences remain between projects in the selected outcome measures. This finding supports the results of the base-year study, which indicated that projects do differ in types of operation and types of students but that when one controls for student input characteristics, differences in outcomes among the various projects are negligible.

Chapter 1

Introduction

I. GENERAL

This document constitutes the final report of an evaluation study of the national Upward Bound (UB) program. The UB program is funded by the U.S. Office of Education (USOE) to help prepare selected low-income high school students with inadequate secondary school preparation to enter and succeed in postsecondary education (PSE). The study, conducted by the Research Triangle Institute (RTI), was supported by the Office of Planning, Budgeting, and Evaluation (OPBE) of USOE (contract number OEC-0-73-7052).

The basic approach taken in this report of the study is to provide material in the text that is general and nontechnical in nature and to present the more technical material and details in appendixes. It is felt that the textual presentation sufficiently describes the basic parameters of the study and study results. The critical reader should refer to cited appendixes or external references to obtain the greater detail that, for purposes of brevity and readability, have not been included in the text.

In this chapter the UB program and the limitations of earlier evaluation efforts are briefly described, an overview of the previous study, hereinafter referred to as the base-year study, upon which the current study is based is provided, and the purpose of and rationale for the first follow-up effort are discussed.

Chapter 2 of this report presents a brief summary of the major findings of the base-year study and is included specifically as background for those not familiar with that study. For greater detail, the interested reader should consult the final report of the base-year study.[1]

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[1] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Programs (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-152)

Chapter 3 presents a description of the methodology of the current follow-up study. Included are discussions of the basic study design, the data bases, data collection, data processing, and general analysis considerations. Constraints and limitations of the methodology in addressing the specific analysis questions are documented in this chapter. Considerably greater detail regarding these aspects of the current study is provided in Appendixes A through G.

Comparative descriptive results addressing differential outcomes for former UB participants and nonparticipants are presented in Chapter 4. The primary mode of data presentation is graphic; however, complete tabular presentations, addressing all analysis questions, are provided in Appendix F.

Results of an expanded approach to analysis are presented in Chapter 5. This set of analyses attempts to capture the existence and magnitude of relationships of student background characteristics, temporal and contextual factors, extent of UB participation, and the interactions of these factors, to student outcome measures. Greater detail regarding the analysis techniques used and results obtained is given in Appendix F.

A summary of the survey findings, limitations, and conclusions is presented in Chapter 6.

II. BACKGROUND

A. The Upward Bound Program

The UB program originated in the Office of Economic Opportunity (OEO) from pilot demonstration projects that operated in the summer of 1965. In 1966, UB was authorized as a national program under Title II-A of the Economic Opportunity Act. In 1969, the UB program was transferred from OEO to the U.S. Office of Education by authority of the Higher Education Amendments of 1968; present legislative authority is the Education Amendments of 1976 (Public Law 94-482).

The program was designed to reach low-income high school students who have potential for successfully completing an educational program but who, due to inadequate preparation and/or lack of motivation, are prevented from meeting conventional criteria for admission to a postsecondary educational program. Through the use of various intervention strategies (e.g., remedial instruction, exposure to new or altered curricula, tutoring, cultural enrichment activities, counseling, and encouragement), the program is legislatively mandated to generate in participants the skills and motivation necessary to enter and successfully complete postsecondary education.

During the summer, UB students typically reside on a college, university, or secondary school campus for an intensive six- to eight-week session, participating in formal course work, attending cultural and social events, and receiving counseling. In the academic year, they typically receive less intensive attention; they may attend Saturday classes, attend periodic tutorial/counseling sessions, or participate in occasional cultural enrichment activities. During their junior and senior years of high school, they receive encouragement and guidance in exploring options for postsecondary preparation in the program best suited to their needs, including assistance in preparing and submitting applications for admission and financial aid.

Institutions sponsoring UB projects are typically two- or four-year colleges or universities; in some cases, projects are sponsored by secondary schools or cooperating groups of institutions. During fiscal year (FY) 1973, or program year 1973-74 (the year in which the national evaluation study was begun), there were 416 UB projects operating in the U.S. and its territories. The UB data bank reporting system indicates that these projects served 51,755 individual participants[2] at a cost of $38.3 million during that period. The UB data system further reveals that, of the 416 projects, 67 were created specifically for veterans, serving approximately 12,200 former servicemen in FY 1973, and 9 were special demonstration projects that served approximately 980 students in FY 1973.

B. The National Evaluation

In July 1973, USOE contracted with RTI to plan and conduct an evaluation of the UB program.[3] Several sources were consulted in designing the study, including the enabling legislation, the program regulations, previous evaluation studies, selected program personnel, current and former UB staff personnel and students, and study advisory panels. This base-year study has been completed, and the results of the study have been submitted to and accepted by USOE.[4] The specific objectives and major design parameters of the base-year study are abstracted in the following section; results of the study are summarized in some detail in Chapter 2 of this report.

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[2] Due to participant turnover and the overlap of program year with fiscal year, the number of participants being served at a given point in time would be considerably less than this figure.

[3] The initial contract also included a national descriptive study of the Talent Search program.

The base-year study was deemed necessary by USOE for several reasons. Studies of the program prior to that time had been inadequate in some respects and were, in most cases, out-of-date. Moreover, the majority of the UB evaluation studies had been performed at the project level and had typically been discounted as "advocacy studies."[5] Although USOE maintains a data bank of information about current and former UB participants, this system was considered inadequate to provide a current, comprehensive evaluation of the program.[6]

The UB program had been subjected to a detailed review and evaluation in 1969 by Greenleigh Associates, Inc., under contract to OEO.[7] The Greenleigh study, however, did not meet the expressed needs at the time of the base-year study in that: (a) the evaluation had been conducted at an early point in the history of the program, reflecting its operation at that time through OEO (substantial program changes had been subsequently made--including transfer to USOE, changes in legislation, and regionalization of program direction); (b) it had not been possible to observe intermediate program effects toward retention of UB alumni in higher education, since few former participants had entered postsecondary education at that time; and (c) certain inadequacies had existed in the control groups employed.

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[4] Burkheimer, et al. (op. cit.)

[5] Davis, J. A. and Kenyon, C. A. Review of the Literature Relevant to the Upward Bound and Talent Search Programs (Volume I of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-150)

[6] Standard available system software is focused on providing project-by-project or aggregate statistics (at various levels of aggregation), but this data source and the related software were neither intended nor designed to provide data to the extent required for a comprehensive evaluation.

[7] Greenleigh Associates, Inc. Upward Bound 1965-69: A History and Synthesis of Data on the Program in the Office of Economic Opportunity. New York: Greenleigh Associates, Inc., February 1970. (ED 046-319)

Another notable study of the UB program was conducted by the General Accounting Office (GAO).[8] Though limited in scope (both in the kind of data employed and in the number of projects examined), this study had raised serious questions as to the relative effectiveness of the UB program and the validity of the information contained in the UB data file, particularly information related to PSE entry and persistence.

III. OVERVIEW OF THE BASE-YEAR STUDY

As previously noted, the base year study has been thoroughly described in a previous report;[9] however, many of the parameters and results of that study impact on the current follow-up study. For the benefit of readers who are not familiar with the base-year report, it is considered appropriate to summarize in this document the salient aspects of the base-year study.

A. Objectives

The primary goal of the base-year study was to evaluate two of the program's major objectives:[10] (1) to increase the high school completion rate of UB participants and (2) to increase the rate of entry of UB participants into postsecondary institutions. Another study goal was to examine project characteristics in relation to attainment of project objectives. Evaluation of the attainment of actual skills and motivation was a secondary goal of the study, primarily because of practical problems involved in determining and measuring the nature and degree of such skills and motivation. Another secondary study goal was to provide a detailed national description of the UB program, including characteristics of the staff and students, their perceptions of the program, and project operations and costs.

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[8] Comptroller General of the United States. Problems of the Upward Bound Program in Preparing Disadvantaged Students for a Postsecondary Education. Washington, D.C.: U. S. Government Printing Office, March 1974. (ED 090-349)

[9] Burkheimer, et al. (op. cit.)

[10] As determined from the mandated program objectives by the Study Advisory Panel.

B. Methodology

Given the objectives, the base-year study focused on the 333 regular UB projects operating in the coterminous United States during the 1973-74 program year.[11] From these regular UB projects 54 were selected after stratification on student ethnicity, number of students served, project location, type of project, and type of host institution. All participants in the sampled project who were in grades 10, 11, or 12 were selected, yielding 3,710 UB students in the final sample. For each selected UB project, an average of two high schools providing students to that project were selected. From a sample of classrooms in each of these schools, a total of 2,340 comparison students (about 21 per sampled school) were selected after stratification on grade level, ethnicity, low-income status, and academic risk. A sample of UB project staff included project directors from all 54 selected projects and a sample of 104 counselors and 211 instructors. In addition, 15 of the 54 sampled UB projects were selected for site visits. (See Appendix A for details of base-year study samples.)

In total, 11 instruments were used in collecting data through questionnaire responses, interview responses, and student records. Three of these instruments were questionnnaires used to obtain information directly from students; four were forms that provided information about students but were completed by teachers, RTI study administrators, or UB project staff members; three were UB project staff questionnaires that were mailed to project directors, project counselors, and project instructors; and the remaining form was a site-visit protocol.

Return rates for the student instruments were generally satisfactory and showed no serious differences over projects. Very low return rates were experienced with only one student questionnaire, which was directed to dropouts, a group that was difficult to locate and probably less motivated to respond. More importantly, for analyses involving differences between UB participants and nonparticipants, return rates did not differ within the two student groups. Although return rates varied considerably for specific student questionnaires, data were obtained through at least one questionnaire or telephone interview for over 98 percent of the students in both the UB and comparison groups.[12]

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[11] The 83 projects operating outside the coterminous United States and/or classified as special veterans or demonstration projects were excluded.

Return rates and differential project response were considered less satisfactory for the staff instruments, particularly in light of the presumed importance of the study to these staff members. Nevertheless, project staff questionnaire responses were considered to be within acceptable levels.

While the response rates defined the upper limit of data availability, they did not provide an accurate picture of the extent of missing data for specific questionnaire items or of the quality of the available data. Consequently, extensive checks for missing data, out-of-range data, inappropriate multiple responses, and inconsistent responses were performed. In attempts to rectify some of these problems, logical and stochastic imputation techniques were employed when feasible. Even so, only a subset of data elements were commonly available due to the diversity of instruments and data collection procedures. A Student Master File, containing those variables that were common to all students, was constructed. This file contained 22 measures on each student, obtained from several sources of student data, and was basic to the data analysis performed in both the base-year and first follow-up studies.

Despite the stratified sampling employed in their selection, comparison students were found to be different from the UB students on several relevant variables. Proportionately, the comparison group included fewer ethnic minorities, more males, fewer poverty level students, and more academic risk students. In addition, the age and grade level compositions of the UB and comparison groups were somewhat different. In light of these differences, statistical adjustments of the comparison students' measures were employed for all analyses.

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[12] A small percentage of these data were collected from secondary sources (e.g., parents, siblings, spouses, high school personnel, or project personnel).

C. Constraints and Limitations

The base-year student data were collected from three student cohorts (tenth, eleventh, and twelfth graders in fall 1973) during spring and fall of 1974. The study was therefore constrained to answering questions concerning high school continuation, college-going rates, etc., only within specific cohorts over a one-year period. Thus, the base-year study collected data on the twelfth-grade cohort only to the point of entry into PSE and did not provide information regarding postsecondary persistence. Other major studies of the UB program (i.e., the Greenleigh study and the GAO study) have also failed to address satisfactorily the matter of PSE persistence. Further, due to temporal data collection restrictions, the RTI base-year study provided only one point (fall 1974) where estimates were obtained for reentry of dropouts into the educational system. Finally, only very general characteristics (e.g., two-year or four-year) of PSE institutions entered by the UB and comparison groups were determined in the base-year study, although a more detailed characterization of the PSE institutions entered (particularly as related to any differential matriculation patterns between the groups) was desirable.

Thus, while the base-year study could provide answers to some major questions regarding the effectiveness of the program, there still remained the need for additional evaluation that would provide answers to other important questions having policy-relevance to USOE.

IV. THE FOLLOW-UP STUDY

During the planning phase of the base-year evaluation study, considerable interest in a longitudinal study was expressed by USOE, RTI, and the study advisory panel. Although beyond the scope of the original study plan, the interest in and potential of a longitudinal study was so great that RTI designed the samples of UB and comparison students to allow a limited longitudinal study in the event that funds might later become available.

In the spring of 1976, RTI submitted, at the request of USOE, a proposal for a first follow-up of the evaluation of the UB program. This proposal, as subsequently modified, was funded on 30 September 1976 as a continuation of the base-year study contract. This follow-up study, the subject of this report, was conducted over a twelve-month period, from October 1976 through September 1977.

As contracted, the follow-up study involved data collection during the winter and early spring of 1977 from members of the base-year study sample. The information collected was minimal and related to the status of study members at that time. Specifically, information collected concerned the following: work status, military status, enrollment status, highest educational level attained and expected, name of current or last-attended institution, problems experienced in obtaining education, UB participation status, and current information as to name, address, and telephone number. A detailed description of the follow-up study design and procedures is presented in Chapter 3. The remainder of this section will describe briefly the purpose of and rationale for the follow-up study.

The primary purpose of the current study was to obtain data on the persistence of UB participants in PSE. The major questions addressed by the study involve the basic issues of educational progress and persistence, and the relationships of these variables to extent of participation in the UB program. The specific major analysis questions, as agreed upon by USOE and RTI, are:

1) Do rates of PSE persistence and of educational progress differ as a function of extent of UB participation?
    a) Are persistence differences between UB and comparison students a function of the level of offering of PSE institutions attended (e.g., two-year or four-year)?
    b) Are persistence differences between UB and comparison students a function of either UB host status or Special Services for Disadvantaged Students (SSDS) host status of institutions attended?
2) For those who were high school seniors in 1973, do the PSE persistence and "lag entry"[13] rates depend upon length of participation in UB? For those who were high school sophomores

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[13] Lag entry is defined as delayed entry into PSE following high school graduation.

and juniors in 1973, does less-than-expected educational progress[14] depend upon the length of participation in UB?

3) Do PSE persistence rate differences depend upon adequacy of financial aid?

4) For those who were high school sophomores and juniors in 1973, do the PSE entry rates differ for UB and comparison students?

5) Do the institutional characteristics (e.g., highest offering, cost, size, residence characteristics, composition of student body, availability of SSDS, UB host status) of PSE institutions entered by UB students differ from the characteristics of PSE institutions entered by comparison students?

The base-year study could not address these questions due to the limited time frame during which it was conducted. Thus, for example, while prior work had established that program participation was related to higher rates of PSE entry, there were no recent reliable data as to whether those UB participants who enter college remain there, which is a more rigorous test of the impact of the UB program.[15] Reflected in the major analysis questions is concern for the fact that during the base-year study estimates of PSE entry were restricted to entry in the year immediately following high school graduation. There was some likelihood that there were differential rates of lag entry between the UB and comparison groups. Depending upon the extent of the difference in this lag entry rate, the findings of the base-year study could have been substantially modified.

There were other reasons for conducting the follow-up investigation. While entry and persistence in PSE are important outcomes for UB, maintained aspirations to continue education are also important intermediate program outcomes. Also of interest are the types of problems experienced by former UB participants and nonparticipants. For those students who do not attend PSE, occupational and military status are also matters of interest. Even though none of the original cohorts were expected to have completed a four-year course of study at a PSE institution (although some could have completed one- or two-year programs), it was considered important to obtain estimates of relationships between UB participation and these other intermediate outcomes. Sufficient time had elapsed to obtain measures of these outcomes for UB participants and nonparticipants alike. Consequently, the following ancillary questions were considered germane to a follow-up:

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[14] Less-than-expected educational progress is defined in terms of expectations of normal uninterrupted progression through high school and postsecondary grade levels, within each grade cohort.

[15] The base-year study provided no data to indicate that the UB participants who entered PSE remained there even through the first term of the first year.

6) Does the current work status of UB participants differ from that of comparison students?

7) Does the current military service status of UB participants differ from that of comparison students?

8) Do the educational expectations of UB participants differ from those of comparison students?

9) Do the problems experienced by UB participants in obtaining their desired education differ from those experienced by comparison students?

A final reason for conducting the follow-up study relates to the amplification and cross-validation of the results obtained in the base-year study. Specifically, due to the one-year time frame during which the base-year evaluation was conducted, estimates of long-term high school completion rates and PSE entry rates were derived from a Markov model employing a synthetic cohort approach. The follow-up study allows an evaluation of the appropriateness of the synthetic cohort approach and also provides direct estimates of high school completion and postsecondary entry for the UB and comparison students who were in grades 10 and 11 during the base year. Since the follow-up was conducted at a time in which all of the original sample cohorts could be expected to have completed high school, given normal educational continuance, the findings of the original study could be considerably strengthened if replicated for different age cohorts.

Chapter 2

Results of the Base-Year Study

I. GENERAL

The base-year study,[1] was designed to address a large number of questions about the Upward Bound (UB) program and its participants. The study involved 54 UB projects, 3,710 UB participants from those projects, and 2,340 comparison students from 106 schools being served by those projects. Additional data were solicited from 54 project directors, 104 project counselors, and 211 project instructors. On-site observations were made at 15 of the projects to obtain a greater insight of day-to-day project operations. The results of the base-year study can be categorized into three basic areas: (a) those describing the characteristics of the UB projects, students, and staff, as existing in program year 1973-74; (b) those comparing student outcome measures of UB participants with those of nonparticipants; and (c) those examining possible relationships between differential student outcomes and project characteristics. Although the results of the latter two categories have more direct implications for the follow-up study, highlights from all three areas are presented to provide background for follow-up study results.

Considerable care was taken in all base-year analyses to avoid approaches that would result in favorable or unfavorable bias toward the UB program, and considerable effort was expended to insure that all data entering analysis were consistent and error free. The sample had been designed to reduce many of the possible differences that could exist between the UB and comparison groups (and which could, therefore, lead to spurious group differences); additionally, statistical equalization of the two groups was undertaken in an attempt to reduce attenuation of results by possible differences not related to UB treatment that were not controlled by sampling. Weight adjustments for instrument nonresponse and item indeterminancy were employed in major analyses in an effort to avoid distortion of the findings due to incomplete data collection. The nature of the steps taken by RTI to obtain meaningful analyses in the base-year study were quite similar to those used in the current follow-up study (see Chapter 3, and Appendixes A through E); the reader who desires additional detail should consult the base-year study report.

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[1] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-152)

Neither the elaborate sampling approaches nor the detailed adjustment procedures used in the base-year study could compensate adequately for two problems that are basic to questionnaire surveys: (1) uncertainty regarding the validity of reported information and (2) the inability to establish causation definitively. These matters impose limitations on both the base-year and follow-up studies and are discussed in some detail in Chapter 3. Other limitations specific to the results of the base-year study are presented below as qualifications of the results.

II. CHARACTERISTICS OF UB PROJECTS, STAFF, AND STUDENTS

A prevailing finding, supported by site visits and analyses of questionnaire responses, was that UB did not appear to represent a single approach to intervention, or even two or three clearly delineated approaches. Variation, rather than commonality, was the salient aspect of aggregate program description for most the the dimensions considered.[2] Within the general limits established by program guidelines, projects varied extensively in the kinds of students served, methods and times of operation, staffing patterns, organization structures, intervention foci, specific intervention strategies used, and the ways in which a given intervention strategy was implemented. While pursuit of the general program objectives appeared to be common across projects, specific objectives and emphases given them showed considerable variation among projects.

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[2] A great deal of the variation among projects during program year 1973-74 probably derives from a combination of two factors: (1) program regulations and guidelines were in a state of flux during this period, and (2) a considerable amount of program monitoring had been regionalized at that time. The Education Amendments of 1972 required the Commissioner of Education to reexamine and resubmit all regulations, policy interpretations, manuals, and general program instructions for congressional review. In the interim period from the appearance of the requirements in the summer of 1972 until the publication of Proposed Rules for the Upward Bound Program in December 1975, the status of the Program Manual and the Upward Bound Regulations of 1970 were in question. Further, the Commissioner of Education delegated funding decisions, proposal evaluations, and monitoring to the Regional Commissioners of Education, effective 1 January 1972. During the period of the study, regional program personnel and projects tended to operate in a relatively flexible atmosphere without effective program regulations or program manual.

A. Project Costs[3]

The estimated average yearly total cost per project (excluding in-kind contributions) was $111,986 for the 1973-74 program. For the 1973 summer program, the estimated cost was $63,769 per project or approximately $830 per student served; for the 1973-74 academic year program, the estimated average cost was $51,863, or approximately $700 per student served.[4] Over 90 percent of these funds came from federal sources. These figures suggest that the average total cost for the 1973-74 academic year program is comparable to the cost of the 1973 summer program. While the summer program was of considerably shorter duration, it was more intensive, involved more full-time staff members, and typically involved more students. There were also additional expenses (e.g., housing and feeding of UB participants on campus) incurred during the summer program.

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[3] Although this aspect of project description was a critical one, the data available for such an analysis were among the most suspect of the base-year study. Attempts were made to obtain official project cost figures, but the data collection effort was ill-timed with respect to federal requirements and insufficient data were received to support analysis. The analyses therefore relied exclusively on the unaudited responses of project directors (or their delegates) to cost items in one of the survey questionnaires. The involved items solicited an extensive complex of fiscal information, which led in many cases to misinterpretations or to complete or partial omissions of details within those questionnaires that were returned. Site-visit verification of these responses during interviews with project directors indicated that all directors questioned had omitted relevant items or had misinterpreted the exact nature of the required fiscal data.

[4] The estimated program year average cost is not equal to the sum of the estimated costs of the two separate program phases. This is because three different regression equations were used in imputing missing values (one each for the summer program, academic year program, and total program year).

There was considerable variation in the cost figures reported for projects. The range of reported total costs, excluding in-kind contributions, was from $9,792 to $175,000 during the summer program and from $19,500 to $134,000 during the academic year. Nonfederal support ranged from $0 to well over $100,000, with the preponderance of projects reporting no nonfederal funding. Projects reported receiving an average of $9,249 worth of in-kind contributions, such as office space, facilities, and personnel services, although these estimates were suspected to be low.[5] Even though some differences existed between the two program components for specific line items, resource allocation to personnel costs, other direct costs (including student stipends), and indirect costs were similar for the academic year and summer phases of the programs.

Examinations of relationships between project costs and program characteristics were undertaken; however, the only stable relationship uncovered was that between the number of students served and total project costs. This result was not surprising, since project funding is determined by a formula which accounts for the number of students to be served. No factors were observed that would suggest institutional or urban-rural inequities in funding.

B. Project Activities and Services

A wide range of courses and classes, tutoring and counseling services, sports, social and cultural activities, and medical and dental services were offered by projects during both the summer and academic year programs. Instruction tended to be oriented toward group discussions or individualized instruction, and competition was deemphasized. Counseling services were offered for a wide variety of student concerns, but, on the average, the most frequent topic of counseling was specific to postsecondary entry. Tutoring assistance ranged from group tutoring to individual sessions and from assistance by other program participants to assistance from university staff members.

Tutoring and counseling services were generally offered by all projects during both sessions, but the availability of remedial, college

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[4] During site visits, clarification of this particular item with project directors indicated underestimation of in-kind contributions in almost every case.

preparatory, elective, and study skills courses varied more widely across projects and between sessions. Except for study skills, specific courses were more likely to be offered in the 1973 summer session but not in the following academic year program, which probably reflects the limited contact with students maintained by projects during the academic year. Corresponding to the universal availability of tutoring and counseling across projects and sessions, tutoring or remedial instruction and counseling were seen by each of the three staff groups questioned (directors, counselors, and instructors) as being by far the most emphasized functions in their projects during both the 1973 summer and 1973-74 academic year programs. Those activities most commonly available were also characterized by the highest participation rates among those students for whom the activities had been available and were also considered to be the most helpful by the students who had participated.

Site visits revealed considerable insight into project operations. While most projects encouraged students to aspire to and prepare for postsecondary education of various sorts, certain projects emphasized personal/social development or completion of high school even to the extent of deemphasizing academic attainment and postsecondary placement. Similarly, some projects left it to the students' maturity and independence to follow through on academic and other responsibilities, while other UB projects were much more structured in this regard.

C. Relations with Host Institutions and Other Supporting Groups

UB staff generally reported receiving at least moderately effective support from their host institutions, their advisory committees, and other parent and community groups. The staff and students reported good relationships among themselves, suggesting that in most projects the directors, staff, and students formed a highly cohesive group. Almost all of the project directors rated their host institutions (primarily public and private colleges and universities) as being supportive. Evidence of host institution support and commitment to specific projects, and to the UB concept in general, was also obtained during site visits.

Directors reported cooperative relationships with other programs for the disadvantaged which operated in their areas (both those administered by the same host institution and those administered by other institutions). UB instructors and counselors also reported receiving a high degree of cooperation from high schools and PSE institutions. Such cooperation is important, since UB projects typically depend on high schools for recruiting students, providing school records, and developing complementary programs of study for students; additionally, the projects depend on PSE institutions for processing applications, granting admission, administering financial aid, and providing for the needs of students in the institutions. Many project directors interviewed during site visits felt the need for more assistance, monitoring, feedback, and direction than they were currently receiving from USOE. A common concern across projects and regions was the timing of notification of funding and consequent late funding, which made definite planning and staffing difficult.

D. Project Staff

On the average, the projects were staffed by one and one-half full-time-equivalent (FTE) administrative employees and three FTE support staff during both the academic year and summer programs. The major staffing difference between the two program components was for instructors and counselors; projects were staffed, on the average, by 4.3 of these service delivery employees during the academic year and by 11.5 during the summer program. There was considerable variation about these average staffing profiles, but no significant associations were found to exist between project staffing patterns and other project characteristics, other than the obvious relationship to number of students served.

Most staff members were young (age 35 or less); nearly all project directors, and over half of the instructors and counselors, were male. The greatest proportion of project directors were black, while the greatest proportion of instructors and counselors were white. Projects appeared to employ staff of the same ethnicity as the student participants, though not always in the same proportions. Most staff members had obtained at least a bachelor's degree, with slightly more than half having obtained a degree at the master's level or higher. In general, the course work and training completed by the UB project staff appeared directly related to their job needs. Over a third of the staff reported current participation in continuing education, and over half had attended workshops on teaching, counseling, or program administration for disadvantaged students. In addition to formal training and education, UB project staff generally had considerable practical experience in their field of work, but less experience working specifically with disadvantaged students. There appeared to be a fairly high turnover of intructional staff, reflecting a program policy of replacing one-third of the instructors each year.

Staff members spent the largest percent of their working time as would be expected; that is, the greatest proportion of the director's, instructor's, and counselor's time was spent on general administration, teaching, and counseling, respectively. However, all staff members performed a number of activities in common, principally, teaching and counseling. Some few staff members reported that their time was distributed among specified activities in a manner that was quite discrepant with an ideal allocation of time. There was, however, no consistent trend for any of the three staff groups to report in the aggregate that more or less than an ideal amount of time was being allocated to any specific activity.

There was close agreement in the ratings of educational goals by project directors, counselors, and instructors. In general, the staff agreed that the more important goals of education were developing student enthusiasm for learning, helping students to feel important, and providing students with a solid grasp of fundamental skills. Instructors rated the following behaviors as most important in their teaching: encouraging students to become involved, giving students praise and affection, answering student questions, encouraging students to make choices, and diagnosing individual learning problems.

E. Recruitment and Characteristics of Participants

UB students most frequently reported first hearing about the program from other UB participants. Other sources from which substantial proportions of participants first heard of the program were school guidance counselors, UB staff members, and school teachers. These results reinforced site visit observations that formal student recruitment was carried out in most projects by "contact counselors" in the feeder high schools. Responsibility for the final selection of students, using various criteria, was assumed by UB project directors and staff. There appeared to be little difficulty or disparity in applying the low-income guidelines in the selection of UB candidates; however, in the interpretation of "academic risk," difficulties of definition were encountered and various procedures were subsequently employed. Project staff relied on specific course grades, grade averages, aptitude test scores, teacher or counselor recommendations, evidences of student motivation (or lack of motivation), and personal intuition in selecting UB students.

About 61 percent of the UB participants were black; 18 percent were white; and the remainder were American Indians, Hispanic, Asian, or unclassifiable. Approximately 56 percent of UB students were female; approximately 85 percent of the students were 16 to 18 years of age; and approximately 15, 39, and 45 percent were in grades 10, 11, and 12, respectively. Based on ninth-grade academic information, which typically reflected performance prior to UB participation, slightly more than half of the UB students were classified as "academic risks." On an index that was closely related, but not identical, to federal poverty-level guidelines, approximately two-thirds of the UB students were considered to be at or below poverty level. About one-half the parents of UB students had attained a formal education equivalent to or greater than a high school diploma. UB students were seen by directors, instructors, and counselors as most proficient in peer relations and creativity. General academic ability of students was rated to be above average by all staff. Students were rated lowest by staff members on their attitudes toward authority and toward school, self-concept, and attention span.

Although most UB students appeared to be the kind for whom the program is intended, the definition of the target group with regard to potential for academic achievement appeared to vary because of lack of specificity, operational feasibility, or differences in interpretation among staff in various projects. Some of the resulting differences in the UB participants among projects may represent a desirable heterogeneity, but this heterogeneity appeared to result from variability in personal convictions or preferences of project staff or from lack of precision in definitions specified in the legislation and in program guidelines.[6]

F. Student Evaluations of UB Projects

Students involved in UB projects appeared positive about the staff and their program experience. The quality of the curriculum, counseling, tutoring, and overall administration was perceived to be quite high, as was the pattern of staff and student interrelationships. The self-reports of the students strongly suggested that they were incorporating program objectives into their own behavior, self-concept, and aspirations. The average ratings of academic year program elements were slightly lower than those of comparable elements in the summer program. Students perceived the UB program's central functions and such day-to-day operations as teaching, counseling, and administration to be well conducted and organized. They generally considered the best qualities of the program to be the staff's interest in the students and the harmonious relationships among the staff and students. They also rated highly the staff's willingness to accept student suggestions. Of the potential benefits attainable from UB participation, students rated gaining a better understanding of the need for education and being prepared to gain admission to college or other types of schools as being most important. At the same time, not all students found all project activities to be helpful.

III. DIFFERENTIAL STUDENT OUTCOMES

To evaluate the attainment of basic UB objectives, a series of analyses focused on comparative student outcomes for UB participants and nonparticipants, or for certain subgroups thereof. Differences between these groups on many relevant factors were reduced by the sample design or by _a posteriori_ statistical equalization of the two groups, as discussed in Section I of this chapter (also see Appendix E). After statistical balancing, the UB and comparison student populations were, in general, quite comparable. Thus, it was reasonable to assume that any differences in outcome measures between the two populations were primarily related to UB participation.

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[6] This is not to imply that ineligible or undeserving students were being served, but that a variety of kinds of disadvantagement were probably represented in different projects.

Since the design used was basically cross-sectional, a problem existed in that analyses examined a long term process at a single point in time. Program participants at that point in time were not only from different age cohorts, but also had received differing amounts of exposure to the UB program. The approach implemented for the base-year study involved the combination of a synthetic cohort design and a Markov model for educational progress.

A synthetic cohort design approximates total process action by examining the input and output at various stages of processing and putting together the various segments to present the total picture. By adopting a Markov model, a theoretical framework based on a transition of individuals through the various stages of the educational process (from tenth-grade entry to entry into postsecondary study), some control over the selectivity of the groups being compared was effected (i.e., control for the comparison of students who "survived" on their own in a school system with the UB participants who had been specifically assisted through the system).

The results of these analyses, which are presented in some detail below, were quite consistent in support of a major central theme: namely, for most outcomes relating to PSE, the UB group differed substantially from the comparison group, but for outcomes not directly related to PSE the groups did not differ. With few exceptions, this theme was pervasive in the results, even for results that may have been attenuated due to excessive or differential proportions of indeterminate data.

A. High School Retention

Two types of high school continuance/completion rates were examined: a fall-to-spring (within-school-year) rate and a fall-to-fall (full-year) rate.[7] Although the fall-to-spring high school continuance/completion rate was greater for the UB group than for the comparison group within each of grade levels 10, 11, and 12, the observed differences were statistically significant only for grades 10 and 11. Regardless of grade level considered, however, continuance/completion rates for both groups were quite high, ranging from 93 to 98 percent. For twelfth-graders, completion rates within the UB participant group tended to increase with the length of time students had spent in the program.

The full-year high school continuance/completion rates were lower for both groups, as would be expected, ranging from 86 to 93 percent. The only statistically significant difference was for grade 10, in which the UB students showed higher rates (93 percent as opposed to 86 percent for the comparison group). Further, these rates did not appear to be related to the extent of prior UB participation. Employing the Markov model for educational continuance, there was no indication that the probability of twelfth-grade completion, given tenth-grade entry, was significantly related to UB participation.

Base-year results thus failed to show consistent relationships between UB program participation and high school completion. For UB participants and comparison students, the estimated probability of completion of any given high school grade was high (86 percent or above), and the estimated probability of completing the twelfth grade, for a student who entered the tenth grade, was about 70 percent, regardless of UB participation.[8]

B. Rate of Entry Into Postsecondary Education

Among high school graduates, 47 percent of the comparison group entered PSE as compared to 71 percent of the UB participants. Among all individuals who could have entered PSE (i.e., those not still in high school, including dropouts), 65 percent of UB students entered PSE as compared to 43 percent of the comparison students.[9] There was also evidence, among high school graduates, that PSE entry rate was positively related to length of participation in the UB program. Specifically, the percentages entering PSE were: 78 percent of high school graduates who had participated in UB in grades 10 through 12, 69 percent of the students who had participated in UB in grades 11 and 12, and 68 percent of the students who had participated in UB only in grade 12. It was estimated that the probabilities of entering PSE for a student who entered the tenth grade were 60.2 percent, 52.7 percent, 47.2 percent, and 31.6 percent, given UB entry in grades 10, 11, 12, or never, respectively.[10]

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[7] Rates reflected continuance for tenth- and eleventh-graders, continuance or completion for twelfth-graders. The fall-to-spring rate was based on a larger number of individuals than the fall-to-fall rate, since the latter required availability of data from the fall 1974 status questionnaire. Procedures employed for fall 1974 data collection involved subsampling of mail nonrespondents (see Appendix A), thereby limiting the number of students for whom these data were available.

[8] This rate was probably depressed for both UB participants and nonparticipants, since the model by which the rate was computed did not allow for reentry of dropouts into the school system.

Although the stated program objectives do not specify placement in any particular type of PSE institution, most UB participants applied to and entered four-year institutions. Of those UB students entering PSE institutions, about 75 percent enrolled in four-year colleges or universities, about 20 percent entered two-year junior or community colleges, and the remaining students entered vocational, trade, or other schools. Analogous enrollment figures for the comparison group were about 45, 30, and 25 percent.

Given equalization (by design and statistical adjustment) of the groups on important background variables, the small number of observed statistically significant differences between the groups on uncontrolled preprocess and extraprocess variables, the virtual lack of practically significant differences on those same variables, and no observed differential response bias on the part of the two groups, there seemed to be only two plausible explanations for differences in PSE entry rates of the magnitude observed. Either the program was facilitating PSE entry for participants, or students were selected for program participation, explicitly or implicitly, on the basis of some characteristic that was highly related to PSE outcomes (e.g., high motivation for educational continuance beyond high school). It was observed that UB participants planned and expected to attend PSE in greater proportions and to progress further in PSE than the comparison group (see following section). This greater motivation for educational continuance after high school should have been directly related to the higher incidence of PSE entry in the UB group, but the question remained as to whether this motivation was effected by the UB program. There was simply not enough available information from the base-year study to determine conclusively whether the UB program was instrumental in raising participants' motivation to attend PSE or whether selection into the program was based to some extent on already extant motivation (site visit observations and interviews did indicate that some projects recruited students on the basis of existing motivation for PSE enrollment).

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[9] These rates reflected entry into PSE within a short time period following high school graduation or dropout. As such they did not account for the possibility of lagged PSE entry and were probably underestimates.

[10] These percentages were probabily underestimated due to basic weaknesses in the Markov model from which they were estimated (e.g., not allowing for reentry of high school dropouts and not allowing for lag entry into PSE).

C. Other Student Outcomes

The extent to which program participation was related to generating, in participants, the skills and motivation necessary for success in PSE could not be directly examined within the design and measurement contexts of the base-year study. An indirect evaluation was conducted, however, by examining differences between the UB and comparison groups on factors theoretically and empirically related to success in postsecondary education. These analyses also served to facilitate interpretation of previously reported results. Specifically examined were: changes that occurred in high school academic measures from ninth grade to current grade (grade point average, proportion of academic credits taken, and academic credits passed); aspirations and expectation for postsecondary education; actions taken in preparation for postsecondary education; and rates of acceptance and receipt of financial aid.

UB participants reported greater motivation for PSE entry and aspired to occupations requiring higher levels of education. They planned to attend PSE in greater proportions and they expected to progress further than nonparticipants. UB participants took more preliminary actions in preparation for PSE; they applied to PSE institutions with greater relative frequency; they applied for financial aid in greater relative numbers; and the aid packages which they were offered were seen as more adequate. While the PSE application rate was greater for UB participants, reported PSE acceptance rates, among those who applied, were no greater for the UB group than for the comparison group. Of those who reported offers of aid packages, UB participants saw the offered packages as more adequate, but there was no evidence that participant aid applicants were offered aid more frequently than nonparticipant aid applicants. The typical aid package reported by UB participants was not statistically greater in value than that for nonparticipants, but it did differ in compositon by including a larger proportion of grant monies as opposed to work-study or loans.

Although these results were somewhat more tenuous than those previously reported,[11] they tended to suggest that the UB program was facilitating actions taken in preparation for PSE. Differences between the two groups in motivation to attend postsecondary institutions were reduced by restricting the analysis to the subset of students planning PSE entry. Because of this restriction, quantitative motivational differences could not provide a plausible alternative explanation of the findings, although qualitative differences in degree of desire to attend PSE may have still existed. While preparatory actions and application rates were higher in the UB group, actual acceptance into PSE among those students who applied was not significantly greater for UB participants. The major function of the UB program in this area, therefore, appeared to be that of aid in the initiation of actions leading to admission rather than aid in gaining admission once application had been made. It should be pointed out, however, that the UB program was working with large proportions of "high risk" students. It is likely, therefore, that the higher application rate among UB participants would have included among the group of UB applicants greater proportions of these high risk students than among the comparison group applicants. If this were the case, then even a similar acceptance rate for UB participants would suggest that the UB program was facilitating PSE admission among those who applied.

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[11] All of the results reported in the previous paragraph were based on analysis of data from subsets of respondents to a particular student questionnaire administered in the schools and projects in the spring of 1974; thus, those who were no longer in the school or project were not represented. Moreover, the extent to which full information as to PSE acceptance or offered financial aid was available to the students at that time is questionable and could well have been differentially available to UB participants who typically enjoyed greater assistance in the application process.

On the other hand, no relationships were found between UB participation and change in high school academic factors. Average increases in grade point average, academic credits taken, and credits passed were quite similar between UB participants and nonparticipants. The failure to find evidence of UB program impact in these measures could have been related to the disproportionately large extent of indeterminate data that obtained or to possibly faulty assumptions that were made in deriving these measures from high school transcripts.

IV. RELATIONSHIPS OF STUDENT OUTCOMES TO PROJECT CHARACTERISTICS

The approach used to explore possible relationships of project characteristics to student outcomes reflected an hypothesis-generating philosophy. The focus of the approach was an attempt to identify any programmatic characteristics that allowed one project to better accomplish program objectives than others, while controlling for differences in students served. Given the reasonably large variation among projects in terms of characteristics and student outcomes, there was reason to expect meaningful results from such analyses.

Three sets of variables were used in these analyses: measures of student characteristics (e.g., student grades prior to project entry, student ethnicity, student poverty status); process measures (e.g., staff characteristics, project functions, type of project instruction); and outcome measures (e.g., PSE entry rate, high school GPA changes, and type of PSE entered). Variables within each class were aggregated over students or staff members, as appropriate, to produce project-level units of analysis. Many of the resulting aggregated process measures were reduced to composite indices through a priori scale construction or factor analysis. The resulting data base produced a set of 54 observations. Several approaches were taken to uncover possible relationships among the three basic sets of variables (input, process, and output), using the statistical methods of multiple discriminant analysis and multiple regression analysis.

The general theme reflected in the majority of analyses was that the characteristics of the students served by a project were consistently related to project level outcomes as measured in the study; however, when differences in student characteristics were controlled, there remained, at best, weak relationships between project process measures and project level outcomes. The relationships between student outcomes and characteristics of the students were fairly intuitive and offered little useful information to UB program planners, since any project could determine the characteristics of its participants through its selection criteria (for which some latitude existed). For example, the fact that a greater proportion of students were placed in four-year educational institutions by projects with proportionately fewer academic risk students was certainly not a surprising finding.

The one small hint of a relationship between process and output came from the frequent occurrence of project size and emphasis of various academic year program functions in the various regression and discriminant models, and the notable absence in such models of measures of emphasis of summer program functions. The total pattern of findings was suggestive of a positive relationship between level of academic year program activity and student outcomes, but there were other plausible explanations of this pattern.

Other relationships between program process and output measures did not appear stable, consistent, or conceptually reasonable. The most predictive relationships between process measures and the individual student outcomes were given by models that were qualitatively different. Such a lack of internal consistency was especially disconcerting in light of the fact that many of the output measures were conceptually similar and correlated. Further, the partial correlations between student outcomes and some project characteristics were negative, suggesting counterintuitive suppressor relationships. Given that one of the dangers of the stepwise analysis techniques used is that they capitalize on chance sampling fluctuations, the inconsistent relationships were dismissed as artifactual.

The failure to discover any systematic set of project characteristics that were related to project success was disturbing and counterintuitive, particularly in light of the latitude allowed in the analyses. It could have been that such relationships did not exist, but lack of observed relationships could be attributed to many other causes--measurement error, inappropriate choice of variables, or inappropriate analysis models, to name but a few. One reasonable explanation is that measurement error and relatively pervasive relationships between input and output jointly mask any existing relationships between process and output.

Another possible explanation of this pattern of negative findings (and one that was supported by observations during site visits) was that input measures and process measures were confounded. That is, different processes were used because different types of students were selected, and different students were selected because a program geared its process to that particular type of student. With this type of confounding, statistical adjustments for input differences, such as those used in these analyses, would have tended to wash out any effects due to process. This explanation was quite consistent with other study findings.

Chapter 3

Follow-Up Study Methodology

I. GENERAL

The follow-up survey of the evaluation of Upward Bound (UB) was a continuation of an effort to obtain information related to educational outcomes of the sample of 6,050 UB and comparison students who were selected for participation in the base-year survey. The evaluation study was initiated as a nationwide, multistage, stratified probability sample of UB participants and nonparticipants in the tenth through twelfth grades in September or October 1973. The follow-up survey consisted of two mailings of a self-administered postcard questionnaire to these sample members and of telephone interviews with a probability sample of 2,500 nonrespondents to these mailings. Collected data were maintained under strict control to ensure confidentiality and to maintain data integrity. While the various methodological and operational procedures of the follow-up study are specified in considerable detail in the Appendixes of this report, this chapter will present a synopsis of those procedures to provide the logical framework within which the findings of the study should be viewed.

The basic design of the follow-up study was obviously constrained by the design of the preceding base-year study. The samples of UB participants and nonparticipants had been drawn during the base-year study (see Appendix A) and many of the variables of major theoretical interest had been defined and measured at that time. Within these constraints, the follow-up study was designed primarily to address questions regarding intermediate student outcomes (see Chapter 1, Section IV), which could not be considered in the base-year study due to temporal limitations, toward providing information that would be useful for assessing and improving current Federal policies and programs concerned with enhancing educational opportunity and achievement of the disadvantaged.

The data collection instrument used in the follow-up study was designed to collect only the information pertinent to specific data needs of USOE, with a minimum burden on the respondent in completing and returning the questionnaire. The medium for collecting the information was a postcard questionnaire that was modified subsequently for telephone interview. Use of a postcard allowed the questionnaire to be folded, sealed (for confidentiality purposes), and placed directly into the mail service once the form had been completed. The specifications for content and format of this instrument were provided by OPBE. Major concerns in preparation of the instrument were: to collect data that would address the study analysis questions; to facilitate response to the instrument; and to maintain clarity and readability of the questionnaire items (to minimize response errors and keying errors). These goals were not always mutually compatible and some compromises were necessary; the resulting instrument is included in Appendix B.

In addition to data obtained from the survey instrument, other data were obtained from existing sources. Student background information, prior school status, and prior UB participation status were available from the student master file developed during the base-year study. Information characterizing the postsecondary education (PSE) institutions attended by sample members was obtained from an institutional data file constructed by the College Entrance Examination Board (CEEB) and from records maintained by the Division of Student Services and Veterans Programs (DSSVP) of USOE.

The operational phase of the follow-up study spanned the period of January through July 1977. Major activities during this period involved data collection, processing, and analysis. To facilitate operations, the entire survey was monitored by a computerized control system from the time of first mailing until an edited machine-readable data base was established. Mailed returns were manually edited for completeness and consistency; if a returned instrument failed the initial edit, a telephone call was made to the individual to clarify or complete the questionnaire responses. Data were then coded into machine-readable form and merged into a master data file (which included data from the base-year study) through machine edit programs. Data from the institutional data file were subsequently merged with those collected through the survey questionnaire.

Two conceptually different, but related, approaches to analysis were undertaken. The first approach can be characterized as comparative descriptive analysis, quite similar in form to the analyses performed in the base-year study. These analyses involved the comparison of educational outcomes of UB participants with those of nonparticipants after statistically equating the two groups on relevant background variables. The second approach can be characterized as relational analysis. Using the individual as the unit of measurement, a general linear model was employed for predicting outcomes on the basis of individual background factors, temporal factors, extent of UB participation, specific UB project association, and the first-order interactions involving those variables.

Throughout the study, care was taken to avoid techniques of data collection, processing, or analysis that would introduce bias into the study results; equal care was taken in interpreting study findings. There are, however, certain limitations imposed on the findings that reflect, in the main, limitations of the data or of the procedures used in conducting the study. The remainder of this chapter is devoted to a more thorough examination of procedures and of data collected, in which both the cautions taken and the limitations are described.

II. DATA COLLECTION PROCEDURES AND RESULTS

This section presents a brief description of the various sequential operations employed in collecting data for the follow-up study as well as the results of those operations. All of these operations were monitored by the computerized survey control system. At each step of the data collection process, computer labels, indicating the step number and a unique code for each sample member involved, were generated to facilitate processing and matching of follow-up data with base-year data. Although response rates were a major concern in data collection, sample coverage rates were considered more important in eliminating possible nonresponse bias.

A. Mail Survey

The first mailing of questionnaires occurred during 27-31 January. A package of materials, consisting of the Follow-Up Questionnaire, a cover letter from the Assistant Commissioner of Education for Planning and Evaluation, and instructions for completing the questionnaire, was mailed to 5,647 of the 6,050 original sample members.[1] A total of 1,461 (25.9 percent) of the first-mailing questionnaires were subsequently completed and returned by sample members.

The second mailing took place on 17 and 18 February. The same packet used previously, augmented by a prompting letter from the RTI project director, was mailed to those who had not responded to the first mailing by that time. There were 4,205 cases involved in the second mailing;[2] of these, 788 (18.5 percent) were subsequently completed and returned by sample members.

Returns from the first mailing began to arrive at RTI in early February. These and subsequent returns from the second mailing could be classified into one of three separate categories: (1) completed or partially completed questionnaires, (2) questionnaires returned as undeliverable, and (3) Mail-Service-supplied address changes. The occurrence of any return was encoded for entry into the survey control system, address corrections were made in the name and address file, and returned questionnaires were processed on a flow basis. Greater detail of the mail survey operation is given in Appendix C, Section II; the questionnaire, with supporting documents, is included in Appendix B.

B. Telephone Interviews

The study design had allowed for a telephone follow-up survey of those individuals who had not responded to the mail questionnaire. It had been anticipated that mail respondents would differ from nonrespondents in terms of the outcome measures being assessed by the study, and coverage of this segment of the original sample was considered quite important. Since cost considerations precluded contacting all mail nonrespondents, a stratified subsample of 2,500 was selected for telephone follow-up. At the time of sampling, questionnaires had been returned by mail from 1,862 individuals, so that the overall selection rate for telephone follow-up was 60.6 percent.

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[1] Of the original cases, 64 had been determined to be ineligible for the survey; additionally, addresses were not available in the name and address file for 339 individuals (see Appendix C, Subsection II.A).

[2] Completed questionnaires had been received from 1,004 sample members, and they were not included in the second mailing. Further, the second mailing was not sent to the 671 cases for whom a questionnaire had been returned as undeliverable. Addresses had been located for 233 of those excluded from the first mailing, and these cases were included in the second mailing (see Appendix C, Subsection II.B).

Actual selection was performed within four strata defined in terms of a crossing of type of nonrespondent (undeliverable or unreturned questionnaire) and availability of certain data from the base-year study; sampling rates were considerably different within these strata.[3] The sampling ensured that all eligible sample members (including those for whom addresses were incorrect or unavailable) had a positive probability of representation in the final data set.

Telephone tracing and interviewing of those selected were begun on 14 March and continued through 6 May, and involved attempts to locate the previous nonrespondents and to collect, by telephone, the basic information on the Follow-Up Questionnaire. During the location process, questionnaire information about the sample member was solicited from family members and other secondary sources. Any information so collected was used if the sample member could not be personally contacted; however, obtaining these secondary source data in no way restricted further attempts to contact the sample member.

Of the 2,500 individuals selected for telephone interview, information was collected (from primary or secondary sources) on 2,298, for a 91.9 percent overall completion rate. The rate was lower (59.3 percent) among those individuals for whom RTI did not have current address information; however, data collection from these individuals was considerably facilitated by a simultaneous tracing operation conducted by DSSVP.[4] Greater detail of the telephone tracing and data collection effort is given in Appendix C, Subsection II.C.

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[3] The formal strata were determined through a joint decision by RTI and OPBE. Cases for whom mailings had been returned as undeliverable and those without available fall 1974 data were undersampled. The reasons for this undersampling were cost related; undeliverable cases were seen as more difficult to trace (and thus more costly), and it was seen as cost inefficient to collect current data from those without fall 1974 data. The procedure used for sampling was a form of systematic random selection, in which additional factors (e.g., base-year sampling factors and student background variables) served as quasi strata. See Appendix A, Section III, for additional detail.

C. Overall Survey Return Rates

Of the 5,973 individuals eligible for the survey,[5] information was obtained for 4,221, for a total response rate of 71 percent. Sample coverage was considerably higher (95 percent) due to the very high contact rate among the mail nonrespondents subsampled for telephone interviews. In general, data availability was greater for the former UB participant group than for nonparticipants, reflecting, in part, the greater ease with which they could be traced through their previous project director; however, differences in data availability among the three grade cohorts were relatively small and were not systematic. (See Appendix C, Subsection II.D for additional details.)

Although sample coverage was not perfect, it was sufficiently complete to assure that any biases in the findings, due to peculiarities of the nonresponding group, would be relatively small. The greater availability of data for former participants, however, raises the possibility that those individuals representing UB nonrespondents are in some way systematically different from those representing the comparison nonrespondents. Similarly, the lower completion rate of telephone interviews among sample members for whom current addresses were not available may also have distorted the results. Weight adjustment techniques (see Section V of this chapter) were applied in an attempt to correct for any biases that may have been introduced by differential data availability.

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[4] This assistance was volunteered by DSSVP as an aid in locating those cases for whom RTI did not have current address information. The tracing operation involved contacting UB project directors by telephone and soliciting their aid in obtaining correct addresses and telephone numbers for previous participants as well as nonparticipants. Contact rates within this student group would have been considerably less without DSSVP assistance.

[5] The final group of individuals determined as ineligible for the study included 13 former sample members who had been reported as deceased.

III. PROCESSING AND MANAGEMENT OF DATA

This section presents a brief description of the steps taken in data processing and management. Greater detail regarding these operations is provided in Appendixes C and D. Although processing activities could not compensate for any deficiencies of the questionnaire items or of the sample members' responses, considerable effort was taken to produce a complete data set that was relatively free of the types of errors often introduced during processing. Wherever possible, operations were performed by or coordinated through a computerized system to reduce the likelihood of human error.

A. Receipt Control and Preliminary Processing

As survey returns were received at RTI, preliminary processing activities were implemented to facilitate survey control and to prepare data for subsequent computer processing and analysis. Although daily receipt logs and tabulations of returns were maintained, primary survey control was maintained through the computerized survey support system (defined in detail in Appendix D). In general, the system maintained data that reflected the current survey status of each sample member, as well as useful historical information describing the sample member's sequencing through each of several defined survey events (see Appendix C, Section IV). To be effective, the system required input for each defined event that took place; therefore, as any of the events occurred for an individual, the event was coded, keypunched, and entered into the system.[6] Since this processing was performed on a flow basis, the events were reflected in the system within a few days of their occurrence.

In addition to providing a convenient and accurate record of survey progress, the support system allowed rigorous quality control of data management activities through a series of internal checks. Such checks involved accessing an individual's current status prior to performing various data entry operations, which considerably reduced the likelihood of entering data for the wrong individual and virtually eliminated the problem of duplicate record entry.

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[6] Some events were computer-generated (e.g., selection of an individual for telephone interview); in such cases, the occurrence of the event was entered directly into the system at the time it was generated.

Some preliminary processing of returned questionnaires was performed manually (see Appendix C). The first step in processing was a scan edit of returned questionnaires, involving certain key questionnaire items. If any of these items were incomplete or inconsistent, then a telephone call was made to the individual to obtain the indeterminate information. Other manual processing steps included additional edits, coding of the questionnaire, and keypunch/verification of data directly from the coded questionnaire. Additional quality control checks were performed on the punched data.

B. Machine Processing

All subsequent processing of questionnaire data was computerized and was accomplished on a flow basis, as detailed in Appendix D. Such processing was undertaken for two purposes: (1) to produce an accurate magnetic transcription of edited Follow-Up Questionnaires and (2) to create meaningful analysis variables from the data of the survey instrument and certain other external sources.

When keyed questionnaire data were available, they were edited and merged into a properly formatted student data file, which had been created from the base-year student master file. Editing consisted of: (1) checking ID number and Federal Interagency Committee on Education (FICE) code of institution last attended for accuracy by means of the associated check digits, (2) checks for out-of-range data, (3) internal consistency checks, and (4) checks for consistency with base-year data.[7] Entry of edited data was performed through the survey control system, and any attempted entry of improper data was rejected. Once student questionnaire responses were entered into the data file, the file was augmented by additional data pertaining to the characteristics of PSE institutions last attended by sample members.[8] This was done by matching the FICE code of the institution indicated by the sample member to an institutional file containing previously constructed indices (see Appendix D).

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[7] Data cards with improper ID or FICE codes were rejected during editing for subsequent resolution; out-of-range data were recoded to an indeterminate value code; and inconsistent data were entered into the data file with an accompanying inconsistency flag.

The final step in data processing involved the creation of analysis variables from the questionnaire responses and base-year data elements. The construction of these variables, detailed in Appendix D, involved the establishment of: (1) various measures of educational progress/persistence from the questionnaire responses and base-year data; (2) measures of other student outcomes from questionnaire responses, and (3) classifier variables for comparative descriptive analyses and predictor variables for relational analyses, primarily from base-year data.[9] A summary of the items available in the final follow-up data file is given in Appendix D.

IV. DATA QUALITY

While response rates and sample coverage define the upper limits of data quality, two related problems of data quality that are typically present in questionnaire surveys are: (a) partial data sets, introduced by incomplete, inconsistent, or otherwise indeterminate responses on returned questionnaires, and (b) questionable validity of responses. This section will address these issues; greater detail is provided in Appendix D.

A. Data Availability

A complete set of data elements was not available for all individuals who responded to the questionnaire. Lack of data could have resulted from indeterminacy within any or all of the three basic data files used in the follow-up study (the base-year student master file, the Follow-Up Questionnaire, or the institutional characteristics file). General availability of base-year student master file data has been documented in Chapter 3 of the base-year study report.[10] Among the 4,221 sample members from whom a Follow-Up Questionnaire was obtained, the relevant base-year variables were available in over 98 percent of the cases, with the exception of poverty status classification, which was available in 91 percent of the cases. Poverty status classification was also the only relevant base-year variable that was differentially available for the two groups, being more available for nonparticipants.[11]

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[8] This was applicable, of course, only for those who had attended a PSE institution. For those who had not entered PSE, a legitimate nonresponse code was entered.

[9] The major exception was extent of UB participation, an index that was derived exclusively from questionnaire responses.

Due to the several efforts to collect complete data during the follow-up study, the proportions of missing data for items of the follow-up instrument were small. Those cases in which items were not completed typically resulted from inability of secondary sources to provide the required information. For the 4,221 cases with completed questionnaires, item nonresponse ranged from 0.7 percent (for highest grade) to 8.1 percent (for educational aspirations).[12] Item inconsistency (either within the follow-up study instrument or between follow-up and base-year data) was also minimal with one exception. Of the four items amenable to consistency checks, all but one showed inconsistency rates of 2 percent or less. The exception was extent of UB participation, for which an excessively large number of inconsistencies existed with a base-year measure of that variable (almost 30 percent). Since this inordinately large inconsistency rate has direct implications for validity, it is discussed more thoroughly in the following subsection.

The availability of information regarding characteristics of the PSE institution last attended was applicable only for those indicating PSE entry and was limited both by indeterminacy of institution last attended (which was less than 2 percent) and by indeterminacy in the external file of institutional characteristics. UB and SSDS host status was available for any specified PSE institution and was thus limited only by indeterminacy of the questionnaire response; these indices were available for all but 35 of the 2,679 individuals indicating PSE entry. All other institutional indices were obtained from the CEEB file, which contained no data for some of the institutions attended by sample members and high rates of data indeterminacy for many other institutions included in the file.[13] Among those sample members indicating PSE entry, data availability rates for these institutional indices ranged from a low of 62 percent for institutional selectivity to a high of 87 percent for institutional level of offering.[14]

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[10] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976 (ED 131-152).

[11] This pattern of differential availability of poverty status classification was also noted and reported in the base-year study.

[12] The specification of the educational aspirations of an individual was a particularly difficult question for secondary sources to address. Nonresponse to all other items of the questionnaire was less than 4.2 percent.

Although for many of the study variables, particularly the PSE indices, data indeterminacy was considerably greater than would be desired, data availability was considered to be generally acceptable. Various statistical approaches were used (see below, Section V) in an attempt to reduce bias that could have been introduced due to variables with excessive indeterminacy rates.

B. Data Validity

Validity of data is a particularly salient concern of evaluation studies that rely on unverified student responses. Invalid responses may be traced to a diversity of possible sources: response bias, deliberate attempts to mislead, lack of understanding or misinterpretation of questions, and simple errors in marking responses. No direct validation of collected data was attempted in this study due to cost considerations; however, two indirect validation efforts were attempted involving examinations of data reliability (reliability is a necessary, but not sufficient, condition for validity). One examination was based on a comparison of responses from duplicate returns;[15] the other involved checks for consistency of follow-up data with base-year data. Both of these investigations of reliability are detailed in Appendix D.

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[13] Exact data availability within the CEEB file is documented in Appendix B of Tenison, L. J. Description and Specifications for the Linked NLS-Institutional Data Base. Washington, D.C.: College Entrance Examination Board, March 1976. The complete data set is available for only a subset of 2,097 of the 4,139 institutions represented in the file. Lack of data in this file actually precluded the development of some institutional indices that had been planned.

[14] This high rate of indeterminacy could possibly have been averted by resorting to additional data of record or by direct survey of the institutions involved; however, the additional cost that would have been involved was a constraint. Even with additional funds, timing would have imposed a constraint, since the subset of institutions attended by sample members was not fully defined until the last questionnaire had been completed.

Examination of duplicate returns generally indicated a high degree of agreement regarding the more objective items (e.g., highest grade completed, extent of UB participation, current enrollment) but considerably less agreement for items that were more subjective (aspirations, problems experienced), reflecting in part temporal instability of responses to the latter items. The agreement between two mail returns was generally greater than that between mail and telephone responses, suggesting some misinterpretation of questionnaire items when an interviewer was not available to aid in interpretation. There was only moderate agreement between student self-reports and secondary source reports. These findings signal caution in interpreting some of the study results.

Consistency of follow-up data with base-year data has been discussed in the previous subsection, and, with the exception of extent of UB participation, no serious inconsistencies existed. Although the inconsistency rate was high for extent of UB participation (almost 30 percent), it may be somewhat deceptive in light of the nature of the inconsistencies and the nature of the base-year measure.[16] Nevertheless, such an excessive rate of inconsistency signals the possibility of a considerable lack of reliability in the retrospective measure of extent of program participation.

The real danger of the validity problem is that of differential validity of responses between the UB and comparison groups. While a lack of reliability and validity serves to introduce error into the data, if this error is the same (regardless of direction or magnitude) within the UB and comparison groups, then group differences will show no directional bias. If, on the other hand, the data from one group are biased in a particular direction and those from the other group are unbiased (or biased in the opposite direction), then the bias will remain (or be magnified) in the group differences. None of the reliability or consistency checks were sufficient to address this issue, and, while there is no reason to suspect differential response biases, the possibility of this source of error in group differences does exist.

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[15] Due to overlapping of the data collection steps, some individuals returned two completed mail questionnaires and some who were interviewed by telephone also returned a mail questionnaire. The subset of individuals with duplicate returns does not, of course, represent a random or representative sample of cases.

[16] Only 100 of the inconsistencies represented a conflict in whether the individual had or had not participated in UB. The majority of conflicts involved the length of participation. It was suspected that these latter inconsistencies were (at least in part) a reflection of the imprecision of the previously derived measure (an index specifying grades in which participation had occurred that had been derived from reported date of first participation).

V. APPROACHES TO AND LIMITATIONS OF ANALYSIS

As previously stated, two different approaches were taken in data analysis, a comparative descriptive approach and a relational approach. The comparative descriptive approach was used to address all of the analysis questions presented in Chapter 1; the relational approach was used to explore the specific contributions of student background factors, temporal and contextual factors, UB participation factors, and their interactions in predicting student outcomes. Each of the approaches used has different strengths and weaknesses, but each approach tends to complement the other. Taken together, the two approaches provide a rather thorough examination of the relationship of UB participation to the intermediate educational outcomes under investigation.

It is important to realize that, while both sets of analyses may show relationships, they do not establish causation. The existence of a relationship between two variables is a necessary condition for the existence of causation, but it is not a sufficient one. In addition to a relationship between two variables, establishment of causation requires that: (a) the causal (antecedent) variable logically precedes the effect (consequent variable); and (b) no other antecedent variable exists which may explain both the antecedent and consequent variables under consideration. While the first of these requirements is often fairly easy to establish, the second is extremely difficult to establish outside the true experimental paradigm.[17]

In examining the UB program, it would be extremely difficult, if at all possible, to identify (much less to measure) all the relevant student, process, and contextual variables which may influence one another, selection of students into the UB program, and/or student outcomes. Moreover, the nature of the sample selection and measurement used in the UB evaluation study was such that it is not possible in all cases to determine even the temporal order of occurrence of two events. Thus, even when strong associations exist between previous UB participation and current student outcomes, the possibility still remains that these associations are largely a function of other unconsidered (or unmeasured) antecedent variables.

To provide unbiased estimates of population parameters, sampling weights (the inverse of the probability of selecting the individual) were used in analyses. The use of sampling weights overcomes possible bias in resulting statistical estimates which may be introduced because individuals were selected with unequal probability (due to oversampling of specific groups). Three conceptually different weight adjustment procedures were used to achieve more meaningful results (see Appendix E). One procedure (standardization or balancing) was used exclusively for comparative descriptive analyses;[18] it effectively forces an equivalence of the former participant and nonparticipant groups in terms of characteristics that are considered to be related to outcomes but that were not completely controlled by the study design (i.e., sex, race, poverty status, academic risk status, and initial grade distribution). The other procedures, used in all major analyses, involved adjustment for subsampling and adjustment for data indeterminacy. The latter procedure, which involves apportioning sampling weights of nonresponding individuals to those of the responding individuals most like them, was chosen over several less conservative imputation approches.

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[17] Although advances have been made recently in the area of causal modeling for nonexperimental or quasiexperimental research paradigms, the assumption of inclusion of all relevant variables in the model is a basic tenet in these approaches.

[18] Since background variables and initial grade were parameters in the relational analysis models, the use of this technique was not required.

While the analysis techniques and their limitations are detailed in Appendixes E through G, they are considered to be sufficiently important in interpreting the findings of the follow-up study that they are discussed briefly in the remainder of this chapter.

A. Weight Adjustments and Loss of Cases

The complex weight adjustment procedure was implemented so that: (1) comparisons between former participants and nonparticipants would not be attenuated by basic background differences between the two groups, and (2) nonrespondents would be represented in the final results by respondents who were most like them in terms of project membership, grade cohort, sex, race, poverty status, and academic risk status. There are, of course, many assumptions that were made in performing these adjustments, and the success of the procedures in attaining the desired control rests on the validity of the assumptions. The problems leading to weight adjustment are, however, those that would have to be addressed in any event, and the assumptions that were made are typically more conservative than those required by other available techniques.

Weight adjustment was a sequential process, and the nature of the basic steps of that sequence is important to an understanding of the basic character of the results of the various analyses summarized in Chapters 4 and 5 of this report. Most steps in the adjustment process involved loss of cases (e.g., those not sampled, those not responding). At each step, the weight resulting from any previous steps was adjusted by a particular technique. Due to the basic differences in the three adjustment techniques used, applying these techniques in different orders would have resulted in somewhat different weights.

The first three steps were quite straightforward. The first step adjusted base-year raw sampling weights for sample ineligibility of 67 individuals. The second step adjusted for the subsampling of mail nonrespondents and resulted in the loss of 1,550 nonselected individuals, while increasing the weights of those selected by an appropriate sampling factor. The third step involved adjustment for failure to contact by telephone and apportioning the weights of the 202 sample members not contacted to individuals most like them.

The fourth step, which did not result in loss of cases, involved producing two balanced weights (one for within-grade comparisons and one for aggregate comparisons) for the comparison group. Since the comparison population had been defined as all non-UB students enrolled in the tenth, eleventh, or twelfth grade of high schools served by UB projects in September 1973, there was no reason to believe that the distributions of this population on relevant background characteristics would be exactly the same as those of the UB participant population. As shown in Table F.40 of Appendix F, the comparison group contained proportionally fewer older individuals, ethnic minorities, females, high school juniors and seniors, and poverty-level individuals. Therefore, any direct comparisons between UB participants and nonparticipants with respect to various outcome measures would not account for the fact that the distributions of the groups being compared were different on these related characteristics. The adjustment involved equating the two groups (participants and nonparticipants) on the marginal distributions of each of several student background factors and on initial grade cohort.

The fifth step in the adjustment process involved the loss of 240 additional "ineligible" individuals, consisting of: (1) those reported deceased by telephone and (2) those with UB participation classification conflicts. The loss of cases with conflicting UB participation classification had not been anticipated, but, once the data were on file, it was determined that some individuals who were originally selected into the UB group now indicated never having participated in UB and that some individuals who were originally selected into the comparison group now stated UB membership (in some cases prior to the original sampling). All such individuals were excluded from analyses.[19]

The final step in the weight adjustment process involved adjustments for indeterminacy of data elements used as outcome (criterion) or classifier (predictor) variables in the major analysis. Adjustment involved apportioning weights of those with indeterminate data to those who were most like them. The loss of cases at this step was typically small, but varied with the particular set of variables considered. It should be noted that this final adjustment was not performed for all outcome variables, particularly those involving characteristics of PSE institutions attended (for which data availability was low). In these and some other analyses, the indeterminate data cases were used as a separate reporting category.

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[19] Original plans had called for inclusion in analysis of individuals originally selected as comparison students who subsequently joined UB; however, on examination of sample weights, it was discovered that the sum of sampling weights for this small group was considerably greater than that for the entire original UB sample. This group was not large enough to be included as a separate analysis category, and, so as not to distort the results, it was decided to exclude this group from analysis.

B. Comparative Descriptive Analyses

The comparative descriptive analyses were analogous to the base-year analyses. Aggregate differences in outcome variables were examined as a function of extent of UB participation, with statistical control of student background factors and initial grade distributions. In general, the comparative descriptive analyses were restricted to the examination of single-degree-of-freedom contrasts of relevant means or proportions for a given outcome measure. Overall tests for main effects or interactions were not employed; rather, meaningful differences in percentages or means between various subpopulations of interest were considered. For each difference an appropriate standard error was calculated to provide a confidence limit for the difference. The results of these analyses reflect the overall relationship between participation in the UB program and student outcomes, controlling for differences in input to the program.

There are some rather obvious limitations to the approach used for these analyses. The approach relies heavily on the efficacy of the balancing technique used for statistical adjustment and is thus bound to the assumptions of that technique. Although statistical adjustments were effected for the set of available measures, there is no guarantee of control over other basic differences between the participant and nonparticipant group. Moreover, when the balanced groups are further partitioned into response-defined subgroups (e.g., those currently enrolled in PSE), there is no reason to expect that the statistical adjustment still holds within any of the resulting partitions. The use of single-degree-of-freedom contrasts, as opposed to omnibus tests, also has disadvantages in that *a priori* contrasts may not fully exploit the available data and may thus obscure other results of interest, particularly interactions.

The exact nature and results of these analyses are reported in Chapter 4, together with major additional limitations specific to the findings. Additional details of the method, rationale, and limitations of these analyses are presented in Appendix F.

C. Relational Analyses

Although the comparative descriptive analyses addressed important questions of overall national impact of the UB program, there were other obviously important questions regarding program impact that could be addressed at a less restricted level of aggregation and control. Specifically, it was considered important to determine if student outcomes were a function of student characteristics (sex, race, poverty status, or academic risk status), temporal factors (age, grade level), or specific UB project association, in addition to extent of UB participation. These factors were effectively adjusted from the results of the comparative descriptive analyses, and thus any "main effects" associated with these concomitant variables or any "interaction effects" of these variables with one another or with extent of UB participation were not assessed. The relational analyses addressed these issues, but the analyses were conducted for only a subset of the outcome measures: educational persistence, PSE entry, and educational aspirations. In conducting the relational analyses, the student was used as the unit of analysis. A general linear model approach was employed which included only main effect terms and some first-order interaction effect terms, since existence, rather than precision of estimates of magnitude, of these interactions was the major concern.

Although the regression approach is quite straightforward and widely used, it is not without limitations. Moreover, most of the standard statistical analysis software packages do not provide routines for correctly estimating the error of regression coefficients in the case of complex stratified survey samples with unequal unit weights. Although RTI has developed a procedure that employs a Taylorized linearization algorithm for better estimation, the number of parameters in some of the regression models tested far exceeded the available degrees of freedom for that approach. Further, the inclusion of specific project membership in any of the regression models precluded the use of the Taylorized linearization approach due to the fact that project membership is completely confounded with the major strata of the design.

The nature and results of these analyses, as well as other limitations specific to the approach and the findings, are presented in Chapter 5 of this report. Considerably greater detail is available in Appendix G.

base-year study. Other analyses were performed for the total groups, aggregated over grade cohorts; for such analyses, additional statistical adjustments were performed for differences between participants and nonparticipants in original grade distribution. For most major analyses (whether within grade cohort or in the aggregate) comparisons were also made within the former participant group on the basis of the number of years (grades) of participation.

This chapter is organized according to the basic issues addressed by the follow-up study rather than strictly in terms of the nine analysis questions. Section II presents results that are related to general educational progress and persistence. Analysis questions 1, 2 and 4 are addressed in that section. The characteristics of the PSE-entering subgroups of participants and nonparticipants and of the postsecondary education (PSE) institutions entered by those subgroups (analysis question 5) are described in Section III. Analyses relevant to such descriptions are necessarily restricted to those individuals who entered PSE.

Section IV presents an examination of postsecondary progress and persistence; this examination is also restricted to the subset of individuals who entered PSE. Analysis questions 1, 1.a, 1.b, and 3 are addressed in that section. Section V presents findings regarding other individual outcomes (work status, military service status, and educational expectations and problems). Analysis questions 6 through 9 are addressed in that section, and analyses are typically not restricted to particular subgroups. Section VI provides a summary and synthesis of the results presented in this chapter. Trends and implications of the findings are discussed in that section.

Although the analyses underlying the findings were relatively complex, those complexities are not detailed in the text. Rather, the results are presented with reference only to their statistical and practical significance. Statistical significance is determined throughout this chapter at the .05 level of chance. To facilitate conceptualization of the findings, the mode of data presentation used in this chapter is graphic rather than tabular; supporting graphic summaries are presented for major findings only. Tabular summaries of all analyses conducted, with accompanying standard errors, are provided in Section IV

Chapter 4

Outcomes as a Function of Upward Bound Participation

I. OVERVIEW

The analysis questions posed in Chapter 1 of this report concern possible relationships between previous participation in the Upward Bound (UB) program and various intermediate educational and work-related outcomes. This chapter addresses those questions. The analysis approach used in producing the findings that are reported in this chapter is comparative descriptive, similar to the approach taken in the base-year study. Analysis plans were jointly specified by RTI and OPBE at an early stage in the follow-up study; these plans were intended to facilitate rather than constrain investigations, and some deviations were realized once data were in hand and analysis had begun. Analysis was constrained, however, by the general design of the base-year study, by the limited set of data collected, and by the specific analysis questions addressed.

Detailed technical information regarding the comparative descriptive analyses is included in the appendixes of this report, and the critical reader may wish to reference one or more of these sources.[1] All analysis results reported are based on weighted data, and thus represent national estimates. In general, the analyses consisted of comparisons between former UB participants and nonparticipants in terms of the specific outcome measures of interest. For such comparisons, various statistical adjustment techniques were employed to control for preexisting differences between the two groups in respect to other variables thought to be related to the outcomes. Many analyses were performed within initial grade cohort, since certain educational outcomes (e.g., highest grade completed) were restricted in the value they could assume by the sample members' educational starting point during the of Appendix F, and reference to the applicable table in Appendix F is given for each set of findings presented.

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[1] Appendix D provides the specifications for developing the analysis variables used; Appendix E provides a description of the various weight adjustment techniques; Appendixes A and E specify the nature of standard error calculations; and Appendix F details the approach, rationale and limitations of the comparative descriptive analyses.

Various general limitations of the findings reported in this chapter have been discussed in Chapter 3 and are detailed in Appendix F. To properly sensitize the reader, however, the major specific constraints on the findings are presented in the remainder of this introductory section.

A. Specific Limitations of Analysis Variables

It should be recognized that the analysis variables used in this study are in many cases rather crude indices of the underlying concepts and that they are constrained by the available data from which they were constructed. Further, the reliability and validity of some variables are questionable (see Appendix D, Section VII). The major issue addressed in this subsection, however, concerns certain theoretical considerations regarding the measures of educational progress and persistence that were used to address the major analysis questions.

A clear distinction between the concepts of educational progress and educational persistence is extremely difficult, particularly for the data set to which variable construction was limited. A measure of educational progress can be obtained; however, such a measure will not be independent of persistence, since persistence is a necessary (but not sufficient) condition for progress. A true measure of educational persistence would ideally take into account an individual's educational goals, but these were not sufficiently obtainable from the current data base. It is theoretically unsound, using the analysis variables constructed, to speak of progress and persistence as independent concepts; since all but one of the measures developed reflect both of the underlying theoretical constructs. The one measure (a weak one) that reflects a particular aspect of persistence is current enrollment.[2]

In addition to the confounding of the theoretical constructs in the measures employed, it should be recognized that the developed measures of progress/persistence are quite time-bound. They may thus reflect not simply progression and persistence in the educational system but also the timing or pace of that progression. The educational pace of an individual may be interrupted (or accelerated) at almost any stage by particular pressures or demands.[3]

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[2] Current enrollment reflects little more than educational continuance and not necessarily any commitment to or attainment of specific educational goals. An individual who is not currently enrolled may be working in order to further his education or may have attained his educational goal already.

The data for this study were collected only three and one-half years after the sample members were beginning their tenth, eleventh, or twelfth grade in high school. Under an ordinary educational pace, this would be ample time for all sample members to have completed high school and entered some form of PSE but not enough time for any of them to have completed a four-year college program. For paces other than ordinary, such assumptions are not valid. On balance, measures of an individual's progress or persistence at a given point in time (especially one no further removed from high school enrollment than was the case here) do not necessarily reflect ultimate educational progress or the persistence toward an educational goal.

B. Specific Limitations Concerning the Relationships Between Extent of UB Participation and Educational Progress/Persistence

As indicated above, most analyses regarding educational progress/persistence involved a partitioning of the former UB participant group on the basis of extent of exposure to the program (1, 2, or 3 years). The relationship between extent of UB participation and educational progress is, however, quite convoluted.

One embroilment that exists is related to an indeterminacy of antecedent and consequent condition. While longer participation in the program may be seen as theoretically preceding further educational progress, this is not necessarily the case. For example, dropping out of high school (cessation of educational progress) leads to termination (and thus limitation) of program participation. The relationship is therefore directionally constrained. This constraint is most pronounced for students in the tenth- and eleventh-grade cohorts.

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[3] For example, it may be necessary for an individual to drop out of the educational system for one or more semesters in order to finance further progress.

Another confounding is related to the so-called "survivor effect," which is a form of group self-selection. As an example, consider the comparison of the educational progress of two groups of twelfth graders; one group consisting of individuals who joined UB for the first time during their senior year, the other consisting of those who participated in UB in the tenth, eleventh, and twelfth grades. Even if the groups are equated on certain background factors, they are different; since one group consists of students who "survived" on their own in a school system through which the other group had been specifically assisted by the program (i.e., to become seniors). Control for the possibility of this effect has not been effected in this study except within the tenth-grade cohort.[4]

C. Specific Limitations of Statistical Adjustments

As specified elsewhere in this report (see particularly Appendix E), a large number of statistical adjustments were performed in the process of conducting the analyses reported in this chapter. In general, the computed standard errors compensated for the assumptions of the adjustment techniques; however, the extent to which adjustments could be made for differences between former UB participants and nonparticipants were limited.

The statistical adjustment for group differences was a process that equated (balanced) the total UB participant and comparison groups on the distribution of certain variables (both within grade and in the aggregate over-grade). One of the requirements of this process is sufficient numbers of individuals within the defined balancing categories to obtain stable estimates for the adjustment. This requirement precluded adjustment within certain response-defined subgroups. Since many of the analyses conducted were only relevant to specific subgroups of sample members, it is important to note that some degree of statistical control was lost in these analyses. The two most frequent subgroups involved were: (1) the subgroups of participants with different length of program participation, and (2) the subgroups of individuals (both UB and comparison) who entered PSE.

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[4] Strictly speaking, this confound is also present for comparisons between UB participants and nonparticipants in grade cohorts 11 and 12. In regards to this particular confound, the Markov model approach used in the base-year study was theoretically more appropriate, despite its acknowledged weaknesses.

While comparisons of the total UB participant group with the nonparticipant group are statistically controlled, comparisons within the UB group (among subgroups with differential exposure to the program) lack this control, since the three exposure groups were not equated on background factors expected to influence educational progress/persistence (e.g., sex, race, poverty status, academic risk status). While in most cases differences among the three exposure groups were minor, they were statistically significant. Groups with greater exposure to the program contained larger proportions of younger individuals and Blacks as well as a smaller proportion of academic risks (see Table F.41, Appendix F).

The subgroup of individuals entering PSE is one of particular interest, since PSE progress/persistence measures were defined for this subgroup exclusively. While former program participants and nonparticipants were statistically equated in a global sense, that statistical equivalence does not necessarily hold after the imposition of another condition (specifically entry into PSE) for further partitioning. In fact, the subgroup of nonparticipants entering PSE differed significantly from the subgroup of former UB participants entering PSE in many respects (see Subsection III.A of this chapter). Comparisons between participants and nonparticipants who entered PSE do not take these differences into account, most of which theoretically favor the comparison group in terms of the progress/persistence measures.

It should be noted that these limitations of statistical adjustment do not invalidate the comparisons; rather, they limit the questions that can be addressed validly by the comparisons. If one wishes to assess differential PSE progress/persistence between former UB participants and previously equalized nonparticipants, notwithstanding the fact that different kinds of students actually entered PSE as a function of having participated in UB, then the comparisons made provide a valid answer. On the other hand, questions involving differences between PSE progress/persistence of former UB participants and an equivalent population of nonparticipants that would have or could have entered PSE cannot be validly addressed by the comparisons made.

II. GENERAL EDUCATIONAL PROGRESS AND PERSISTENCE

Overall persistence in the educational system and progress through high school and postsecondary education are addressed in this section. The findings presented involve differences between all former UB participants and nonparticipants in respect to various measures of educational progress/persistence (i.e., highest educational levels attained, high school completion rates, delayed entry rates, less-than-expected rates of educational progress, and rates of postsecondary entry). In terms of the specific analysis questions posed in Chapter 1, this section addresses questions 1, 2, and 4.

A. Highest Grade Completed, High School Completion, and Postsecondary Entry

A relatively straightforward definition of educational progress is in terms of the highest level of education completed after a certain length of time. Figure 4.1 presents a comparison between the reported current[5] highest education of former UB participants and that of nonparticipants (see also Table F.1.5); the figure describes the total UB and comparison groups in aggregate over all grade cohorts. The consistent trend evident from Figure 4.1 is that former UB participants show greater progress than nonparticipants. At any level of highest grade completed (excluding the maximum value), the cumulative percentage of former participants who are limited to that level or less is smaller than the corresponding percentage of nonparticipants. For example, the percentage of nonparticipants who have completed no more than one year of college is about 80 percent; the corresponding percentage for former UB participants is about 70 percent.

Because the initial grade level (tenth, eleventh, or twelfth grade in fall 1973) differentially limits the highest grade level attainable by the time of this study, the levels of attained education differed between grades; however, within any grade considered, the basic pattern was the same--former UB participants showed greater educational progress

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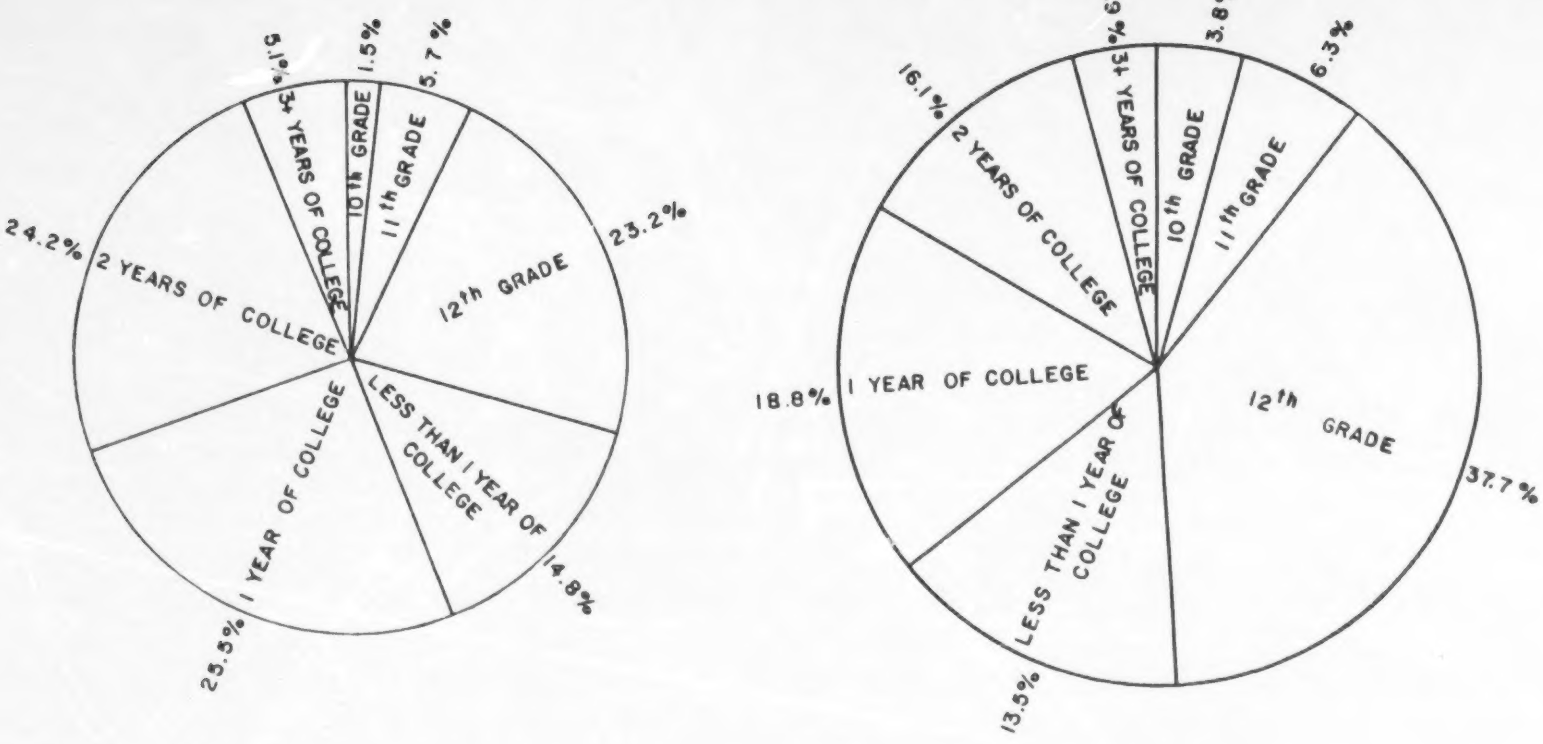
[5] Throughout the presentation in this chapter the term "current" is used to mean at the time of data collection, which spanned the period of February through April 1977.



FORMER UPWARD BOUND PARTICIPANTS

NONPARTICIPANTS

Figure 4.1. Current highest educational level obtained by former Upward Bound participants and nonparticipants (all grade cohorts)

than the comparison group. Since highest grade percentage breakdowns by grade are redundant in pattern and since other reported analyses adequately reflect them, they are not presented. The interested reader can, however, refer to Tables F.1.2 through F.1.4 in Appendix F for percentage distributions and summary statistics by grade.

If the length of UB participation is considered, within the former participant group, the general pattern observed is that greater participation in the program is typically associated with greater educational progress. This pattern held within each of the three grade cohorts. The actual percentage distributions of highest grade completed by extent of participation are not presented in this chapter, since the results of other analyses reported below adequately reflect such differences. These percentage distributions, together with various summary statistics, are available in tabular form in Appendix F for both within-grade (Tables F.1.2 through F.1.4) and aggregate (Table F.1.5) comparisons.

Given the observed differences in highest grade completed, as reflected in Figure 4.1, it is considered desirable to summarize the differences for statistical tests. There are many ways available for summarizing these data; however, only two basic approaches are used here: (1) representing highest grade completed by an average value, and (2) dichotomizing highest grade completed into categories that represent meaningful educational milestones.

1. Average Highest Grade Completed

Figure 4.2 presents the mean highest grade completed for the UB and comparison groups by grade cohort and by extent of UB participation (see Table F.1.1 for additional detail).[6] For those former UB participants in the tenth-grade cohort, the average current highest grade completed was approximately 12.2 as compared to an average highest grade of 11.9 for nonparticipants in the same grade cohort. Although the actual values of average highest grade completed differed, as expected, from cohort to cohort, the advantage of former participants, by approximately three-tenths of a grade level, over nonparticipants is also

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[6] In scaling highest grade from the responses to the questionnaire item, an individual who indicated only completing grade 12 but who also indicated the last school attended was a PSE institution was assigned a score of 12.5. For other details of this scaling, see Appendix D.

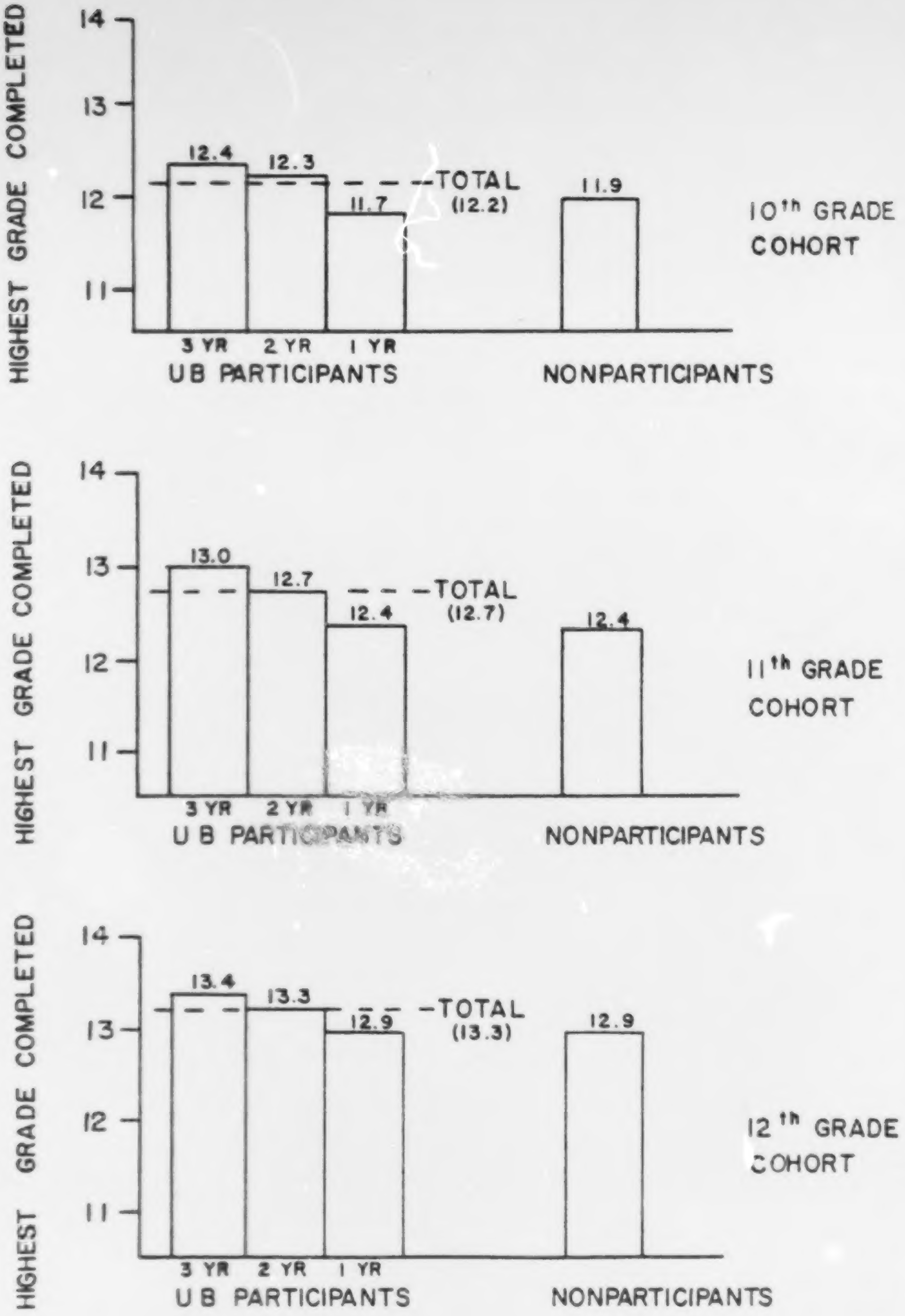

Figure 4.2. Average highest grade completed by former Upward Bound participants and nonparticipants by grade cohort

evidenced for the eleventh- and twelfth-grade cohorts. These within-cohort differences were statistically significant, as were aggregate differences.

Figure 4.2 not only demonstrates that the former UB participants complete a higher level of education on the average, but also that the length of UB participation is positively related to the average highest educational level completed.[7] Within each grade cohort, one-year former participants were contrasted with two- and three-year participants, and two-year participants were contrasted with three-year participants in terms of average highest grade completed.[8] With but one exception, all such within-grade contrasts (as well as similar contrasts for over-cohort aggregation) were statistically significant.[9]

2. High School Completion Rates[10]

Another way of summarizing highest grade completed into a measure of educational progress/persistence is to dichotomize the responses into those reporting less than twelfth grade completion and all others, thus determining the rates at which UB and comparison students complete high school and/or enter PSE. This index also represents minimal educational persistence through at least high school completion. High school completion rates by grade cohort and by extent of UB participation are presented in Figure 4.3. The relative numbers of individuals who persisted through the twelfth grade or higher was approximately the same for former UB participants and nonparticipants except within the eleventh-grade cohort. For that cohort, the greater rate (by 7 percentage points) within the former participant group is a statistically significant

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[7] Recall, however, that the relationship between educational progress and extent of UB participation is directionally constrained and that the various UB participation subgroups are not strictly equated on background variables, as specified in Subsections I.B and I.C of this Chapter.

[8] Except as otherwise noted, these specific single-degree-of-freedom contrasts were used for all comparisons within the former UB participant group.

[9] The exception was for the difference between two-year participants and three-year participants in the tenth-grade cohort.

[10] Although the term "high school completion" is used for convenience of expression, this does not necessarily imply high school graduation, per se, since PSE entry does not in all instances depend on high school graduation.

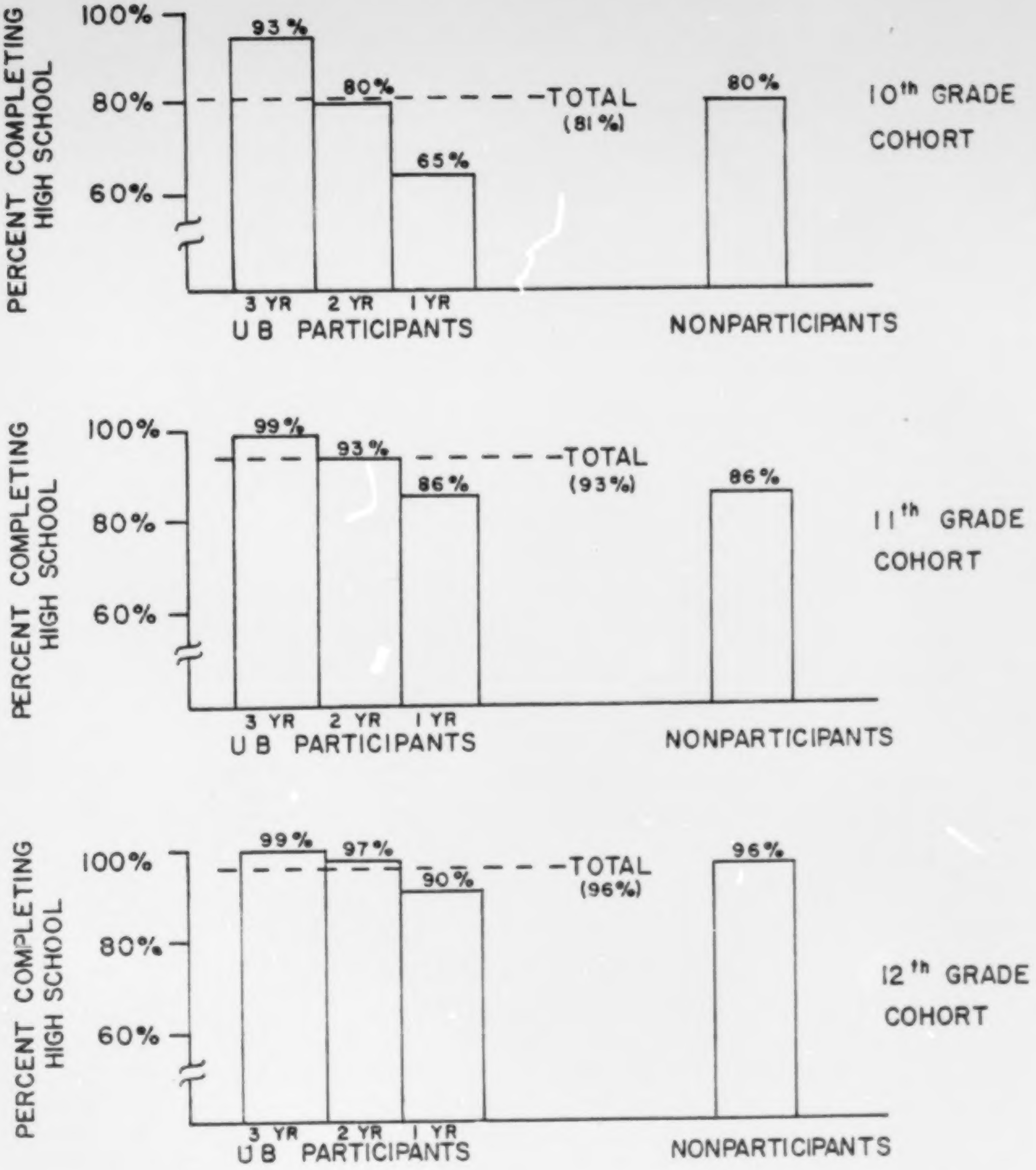

Figure 4.3. High school completion rates for former Upward Bound participants and nonparticipants by grade cohort

difference. Aggregate high school completion rates (see Figure 4.1) for former participants and nonparticipants were 93 percent and 90 percent, respectively, which did not approach statistical significance.

These results are not in complete agreement with results of the base-year study; however, the two sets of results are not strictly comparable in many respects. Base-year findings showed twelfth graders completing high school at somewhat lower rates than those presented here, but the base-year results could not allow for reentry of dropouts into the system. These results were consistent with those from the base-year, however, in indicating no differences between twelfth-grade cohort participants and nonparticipants in the rates of high school completion. High school completion for the tenth-grade cohort, as predicted by the Markov model used in the base-year study, was underestimated for both UB participants and the comparison group, as compared to the results reported in Figure 4.3 (the Markov model also failed to allow for reentry of dropouts). For students in grade ten, the Markov model had predicted a 75 percent high school completion rate among UB participants and a 70 percent high school completion rate for nonparticipants. Actual completion rates for the tenth-grade cohort at the time of this study were 81 percent for former participants and 80 percent for nonparticipants. These findings were again consistent with the base-year results in indicating no significant differences between the groups.

High school completion rates, as shown in Figure 4.3, were also a direct function of length of participation within the former participant group. The differences among groups with differential exposure to the program is most pronounced within the tenth-grade cohort; however, as specified in Section I of this chapter, such comparisons within the tenth- and eleventh-grade cohort may be misleading. Contrasts were made within the UB group similar to those reported previosuly. Within each of the grade cohorts, the one-year former participants showed significantly lower high school completion rates than the greater-than-one-year former participants, but only within the eleventh-grade cohort did the two-year participants differ significantly from the three-year participants.

The differences in high school completion rates over the three grade cohorts probably reflect the longer time period over which high school attrition can take place in the earlier grades. Another influencing factor may be that insufficient time has elapsed for any correcting effects of dropout reentry within the tenth- and eleventh-grade cohorts (see Table F.2 for greater detail).

3. Postsecondary Entry Rates

Another educational milestone is entry into postsecondary education; highest grade completed was again dichotomized to reflect an index of educational progress and persistence through entry into PSE. PSE entry rates for former participants and nonparticipants by grade cohort and by extent of program participation are presented in Figure 4.4. Within each of the grade cohorts, former UB participants have entered PSE in greater relative numbers than nonparticipants. PSE entry rates for UB participants were consistently 16 to 18 percentage points greater than those for the comparison nonparticipants; all such differences were statistically significant. PSE entry rates are obviously different over grade cohorts, probably reflecting greater likelihood of high school attrition in the earlier grades and the timing of educational progress (e.g., reentry of high school dropouts and delayed PSE entry--see Subsection II.C below).

While the follow-up results concerning PSE entry are not strictly comparable to the base-year findings, such a comparison is of interest. Base-year PSE entry rates for the twelfth-grade cohort high school graduates were 71 percent and 47 percent for former participants and nonparticipants, respectively. These are seen, in light of current results, to have been underestimates for both groups, and reflect the fact that delayed entry occurred in both groups (see Subsection II.C below). The Markov model was also used in the base-year study to predict PSE entry rates for UB participants and nonparticipants. The model differentially misrepresented PSE entry for the UB and comparison groups. The model predicted 60 percent PSE entry rates for UB participants in the tenth-grade cohort and 32 percent PSE entry rates for nonparticipants in this cohort. Empirical findings show these rates to be 55 percent for participants and 39 percent for nonparticipants. The model conveniently assumed, however, that a UB participant who continued in high school would also remain in the program; that assumption was not supported empirically (as reflected in Figures 4.3 and 4.4), which probably explains some of the differential misrepresentaton of the model predictions.

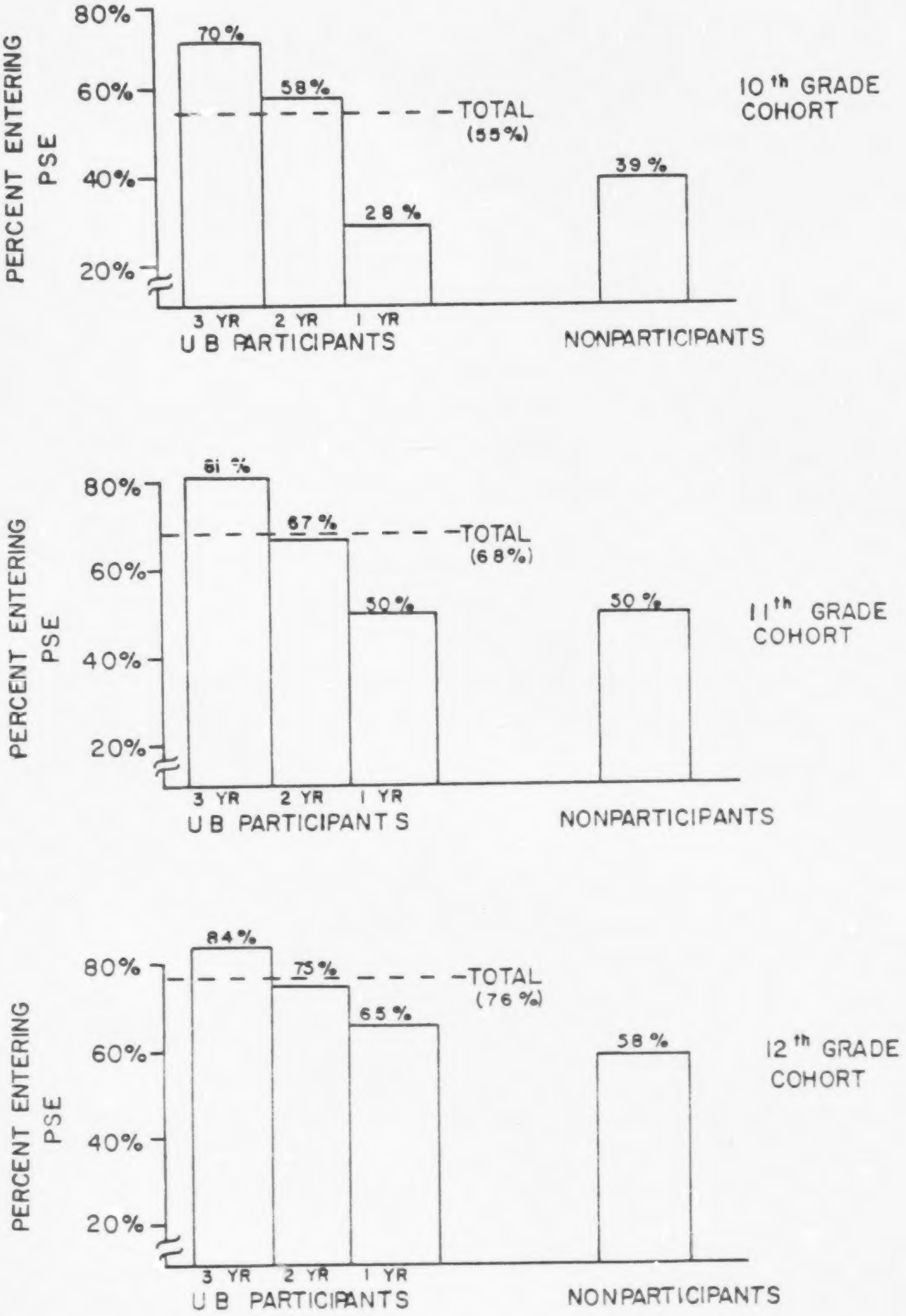

Figure 4.4. Postsecondary entry rates for former Upward Bound participants and nonparticipants by grade cohort

Figure 4.4 also shows a positive relationship between length of participation in the UB program and PSE entry within each of the grade cohorts, which again requires care in interpretation for the tenth- and eleventh-grade cohorts. Contrasts analogous to those previously specified indicated that PSE entry rate differences between one-year participants and greater-than-one-year participants were in all cases statistically significant. Differences in PSE entry rates between two-year and three-year participants were also statistically significant in all but the tenth-grade cohort. The pattern of these findings were the same as those reported in the base-year study (see Table F.3 for additional detail).

Another way of examining PSE entry rates is to consider the rates within the subset of individuals who reported at least high school completion. While such a presentation is based upon response-defined subgroups (with some attendant loss of statistical control) it is more meaningful for comparing PSE entry rates within the UB participant group, since the directionally constrained relationship of high school completion to extent of UB participation is removed from the results. The PSE entry rates for former UB participants and nonparticipants who reported at least high school completion are presented in Figure 4.5 (Table F.4 provides additional detail). As can be seen from the figure, UB participants enter PSE at higher rates after high school completion than do nonparticipants. For any grade cohort considered, entry rates for former participants are from 16 to 19 percentage points greater; all such differences are statistically significant. Since these PSE entry rates no longer reflect high school attrition, differences among the three grade cohorts are probably a function of delay in PSE entry (see Subsection II.C). In support of that hypothesis, it is interesting to note that the entry rates of participants and nonparticipants within the tenth-grade cohort (who should have completed high school the spring prior to the follow-up study) are quite similar to the base-year study rates for those in the twelfth-grade cohort (who had just graduated at that time). Base-year rates were 71 percent and 47 percent for twelfth-grade participant and nonparticipant high school graduates, respectively.

As noted above, comparisons among UB participants with varying extent of exposure to the program are more meaningful for the results

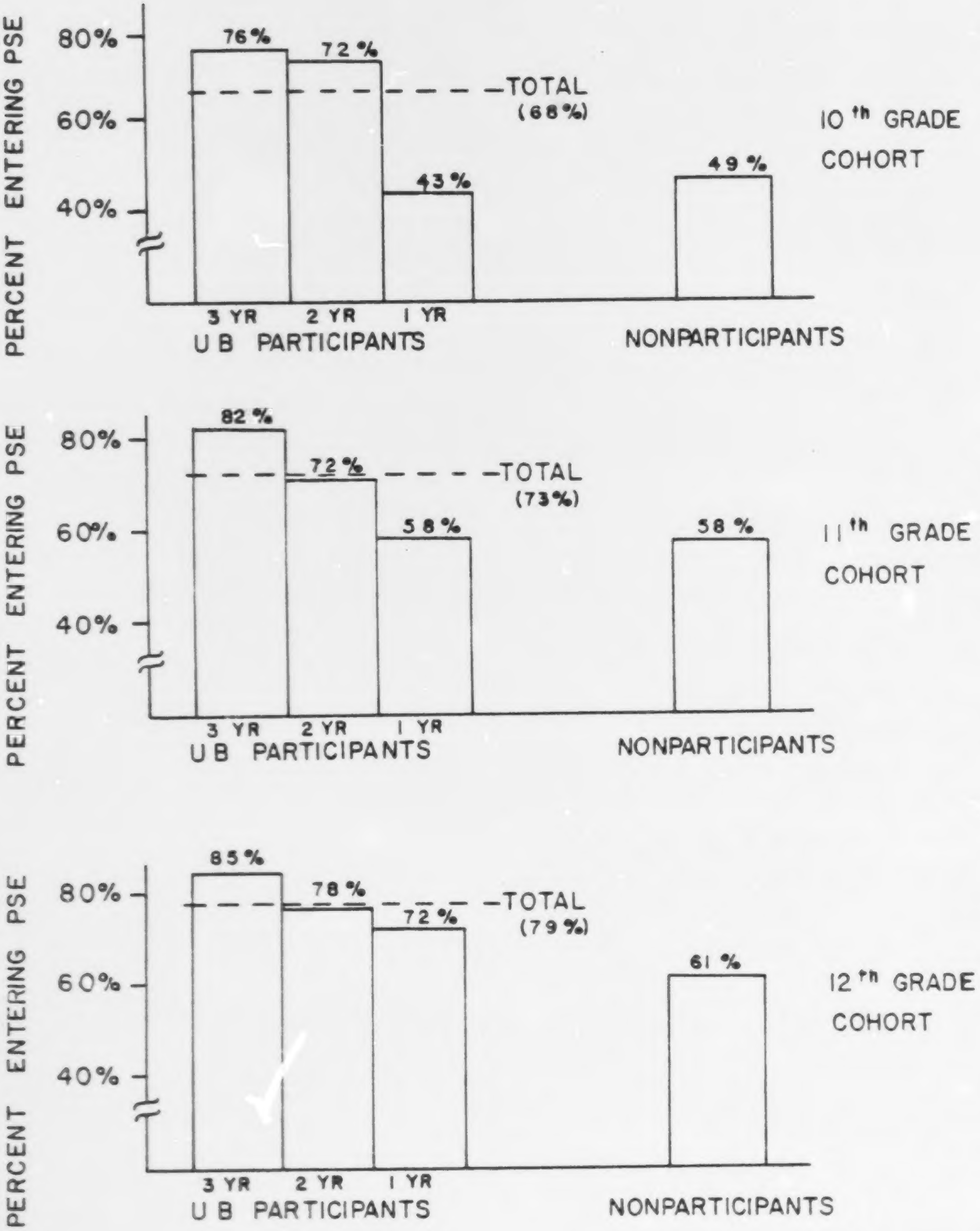

Figure 4.5. Postsecondary entry rates for former Upward Bound participants and nonparticipants who indicated at least twelfth grade completion, by grade cohort

depicted in Figure 4.5. The positive relationship between extent of participation and PSE entry is obvious within each of the grade cohorts. Contrasts among the three exposure groups indicated that *statistically significant differences existed within all grade cohorts between one-year participants and greater-than-one-year participants and that for the eleventh- and twelfth-grade cohorts, three-year participants entered PSE at significantly higher rates than two-year participants*.

B. Additional Consideration of Differences within the UB Participant Group

In Subsection I.B of this chapter certain limitations were discussed regarding comparisons of general educational progress/persistence within the former UB participant group. The basic point of that discussion was the difficulty in determining whether termination of UB participation preceeded or followed termination of high school progress. The only findings presented to this point that are relatively free of this partial confounding are results within the twelfth-grade cohort and the PSE entry rates that are conditional on high school completion (Figure 4.5). For such findings, results still indicate greater educational progress/persistence as a function of length of UB participation. Although this entanglement in interpretation cannot be fully resolved, the pattern of results previously presented and additional analyses that were conducted do shed some light on the matter.

Figures 4.2 through 4.4 in Subsection II.A above, show a of *differences* in outcome measures between three-year and one-year UB participants as one considers successively the tenth- through the twelfth-grade cohort. As an example, from Figure 4.3 the difference in high school completion rates between one-year and three-year participants is 28 percentage points within the tenth-grade cohort, 13 percentage points within the eleventh-grade cohort, and 9 percentage points within the twelfth-grade cohort. Granted that there are ceiling effects for the three-year participants, one reason for this pattern could be related to the type of confounding suspected.

In order to further investigate this issue, additional analyses were undertaken. The individuals chosen for such analyses were those with equal extent of UB participation but with differing patterns of participation, specifically the two-year participants in the tenth- and eleventh-grade cohorts.[11]

Indices of educational progress for former two-year UB participants were compared within the tenth- and eleventh-grade cohorts as a function of whether or not they had participated during the twelfth grade. The pattern of results were quite straightforward and held for each of the measures examined (average highest grade completed, high school completion rate, and unconditional PSE entry rate). Within each cohort the two-year participants who had participated in the twelfth grade (e.g., participation patterns of tenth and twelfth grades or eleventh and twelfth grades) demonstrated educational progress/persistence quite similar to the three-year participants within the same cohort. Those who had not participated in UB in the twelfth grade (e.g., participation pattern of only tenth and eleventh grade) demonstrated educational progress/persistence much like that of the one-year participants within the same grade cohort. All differences, within grade cohorts ten and eleven while controlling for total program participation, between those who had participated in the twelfth grade and those who had not participated in twelfth grade showed significantly greater educational progress/persistence for the twelfth-grade-participating group (see Table F.5 for greater detail).

There is, therefore, some additional support for the proposition that differences in general educational progress/persistence between different UB exposure groups within the tenth- and eleventh-grade cohort may be somewhat artifactual.[12] The positive relationship between extent of UB participation and educational progress/persistence cannot, however, be completely discounted due to the findings within the twelfth-grade cohort and the findings regarding PSE entry conditional on high school completion, both of which are free from the type of confounding examined in this section.

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[11] Three-year and one-year participants could not show different participation patterns since three-year participants must have been in UB during all three grades (tenth, eleventh, and twelfth) and since one-year participants were in UB for that particular grade only.

[12] There are, of course, other explanations for these findings; for example, there are some things that are unique about UB participation in the twelfth grade. For one, that is the grade during which the greatest program emphasis is given to assistance in applying to PSE. While this could be expected to influence PSE entry rates, it would not be expected to influence high school completion rates. Thus, the explanation advanced in the text seems to be the most straightforward one; many of the one- and two-year UB participants did not participate in the program in the twelfth grade because they did not enter the twelfth grade.

C. Examinations of Nontypical Education Patterns

1. Delayed Postsecondary Entry

One of the drawbacks to the base-year study was the inability to estimate educational progress that was made through other than normal uninterrupted progress. The extent to which those in the twelfth-grade cohort delayed entry into PSE after high school graduation could very well have changed the base-year findings concerning PSE entry rates. Delayed PSE entry (lag entry) has also been advanced as a possible explanation for some observed progress/persistence differences among the different grade cohorts. The follow-up study provided a means of investigating delayed PSE entry, by examining the current PSE entry rates for those in the twelfth-grade cohort who had not entered PSE during the period of the base-year study. Within this subset of individuals any current indication of previous PSE enrollment would necessarily have been delayed entry.

Figure 4.6 shows the estimated percentages of former UB participants and nonparticipants who did not enter PSE immediately after completion of high school but who did indicate subsequent (delayed) entry into PSE. The values were computed for those in the twelfth-grade cohort during academic year 1973-74 who did not enroll in PSE in the fall of 1974. The results indicate that, of those individuals who did not enter PSE immediately after high school, a greater proportion of former UB participants subsequently entered than did nonparticipants. The difference in lag entry rate, which is statistically significant, bolsters the findings of the base-year study. Not only do UB participants enter PSE at greater rates than the comparison nonparticipants immediately following high school, but also the subsequent rate of lag entry (among those who do not enter immediately) is also greater for UB participants.

The extent of UB participation is also seen to show a positive relationship to lag entry. Although the absolute increments in lag entry over the various extent-of-exposure groups were large, these analyses were based on such small numbers of individuals that sampling

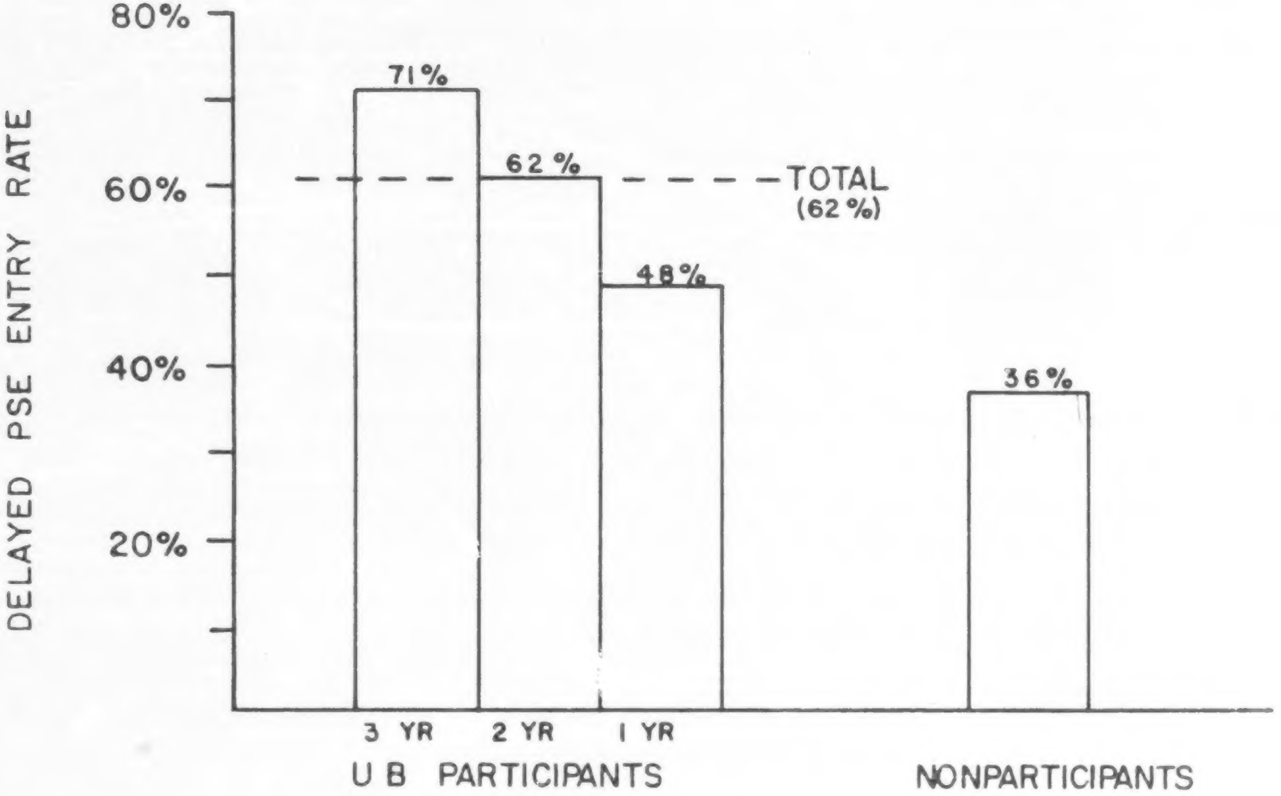

Figure 4.6. Postsecondary entry rates among former Upward Bound participants and nonparticipants who did not enter immediately following high school completion (twelfth-grade cohort only)

error was also quite large (standard errors of difference of 6 to 9 percentage points), and the two contrasts among these groups were not statistically significant (see Table F.6 for details of the analysis regarding lag entry).

2. Less-Than-Expected Educational Progress

One of the difficulties in examining educational progress among the various grade cohorts has been the fact that within the limited amount of time since the base-year study, there are differential limits on the educational attainment possible. A way in which this problem can be partially surmounted is by examining educational progress in relation to expectations of normal uninterrupted progression through high school and postsecondary grade levels. This was done for those who were high school sophomores and juniors in 1973.

Figure 4.7 shows the percentages of former UB participants and nonparticipants demonstrating less-than-expected educational progress[13] within grade cohorts 10 and 11. The figure indicates comparable results for former tenth and eleventh graders, with only slightly greater rates of less-than-expected progress for the tenth-grade cohort. This probably reflects the greater likelihood of high school attrition given the lower educational starting point for tenth graders. Within both grade cohorts, significantly smaller proportions of UB participants than comparisons (by about 18 percentage points) demonstrated interrupted progress through high school and postsecondary grades. Length of UB participation is also directly and significantly related to less-than-expected educational progress; greater participation in UB is associated with lower rates of interrupted education for both cohorts (see Table F.7 for greater detail).

III. TYPES OF INDIVIDUALS ENTERING POSTSECONDARY EDUCATION AND TYPES OF INSTITUTIONS ENTERED

The results concerning overall educational progress and persistence, presented in the previous section, were straightforward and consistent

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[13] Less-than-expected progress was defined as less progress than would be expected with normal educational progression and reflected some form of interruption of educational progress. For those in the eleventh-grade cohort, expected progress was completion of one year of PSE. See Appendix D, Section VI for details.

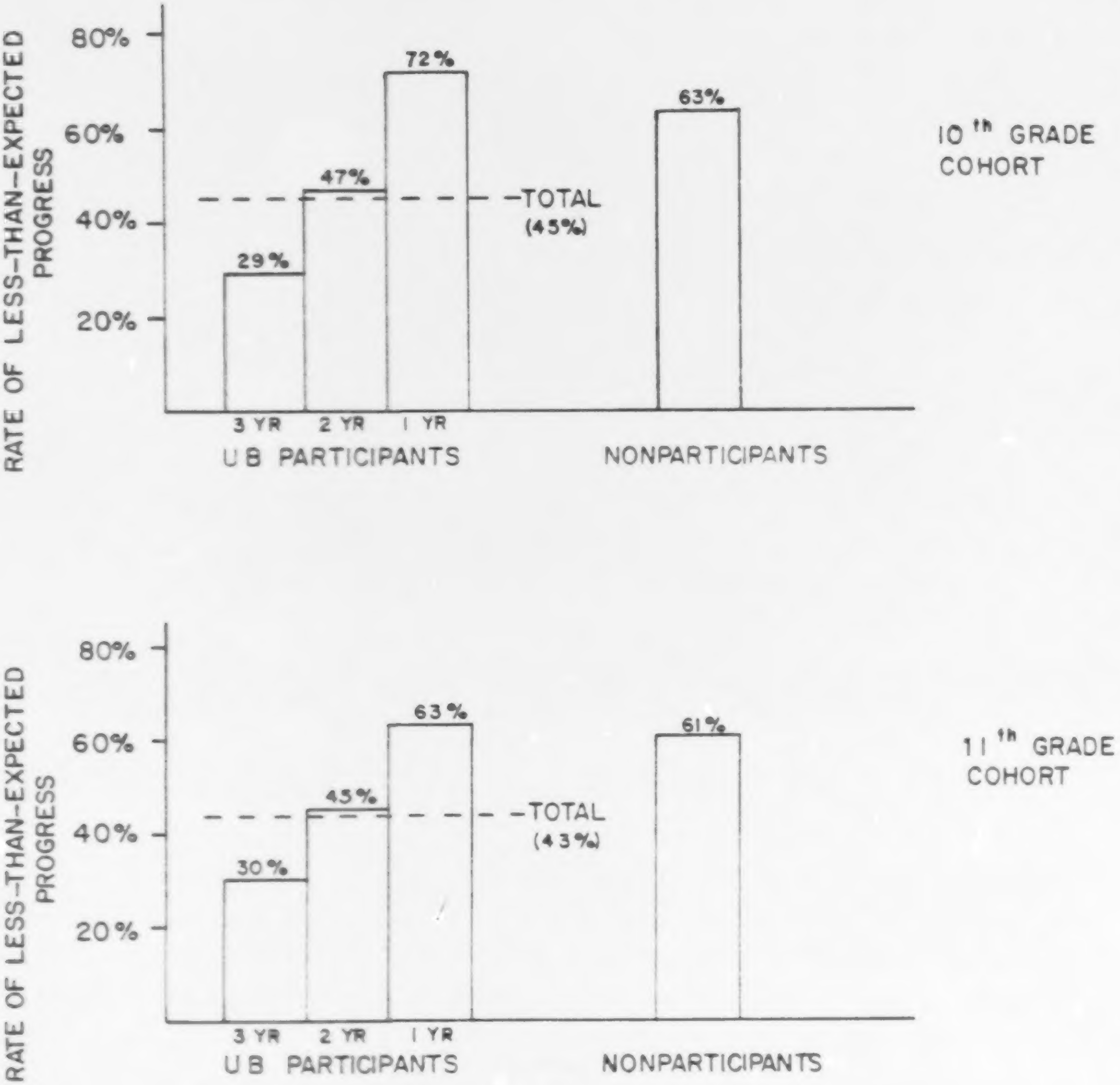

Figure 4.7. Rates of less-than-expected educational progress for former Upward Bound participants and nonparticipants (tenth- and eleventh-grade cohorts)

in indicating greater progress among former UB participants than among a statistically equated comparison group of nonparticipants. The differences can be at least partially attributable to the greater rates at which former UB participants enter PSE, since systematic high school completion rate differences between the two groups did not exist. A logical extension of the investigation of relationships between educational progress/persistence and UB participation would be to examine the progress and persistence of individuals after they have entered college or some other kind of postsecondary school. Possible moderations of any PSE progress and persistence differences at different types of PSE institutions are also matters of obvious concern.

Prior to considerations of PSE progress and persistence (which are presented in Section IV below) it is important to determine the types of participants and nonparticipants who entered PSE. Also of relevance to PSE persistence are possible differences in the types of PSE institutions entered by the two groups. These issues are the topic of this section; in terms of the specific analysis questions posed in Chapter 1, this section addresses question 5.

A. Types of Individuals Enrolling in Postsecondary Education

In investigating possible differences between the PSE-entering subgroups of former UB participants and nonparticipants, the comparison population considered was the statistically adjusted one, which had been equated to the UB participant group in terms of various background factors. Thus, any UB-comparison differences on these background factors within the subgroup entering PSE reflect differential educational attrition, in respect to those factors, as a function of UB participation.

Figure 4.8 shows differences between PSE-entering UB participants and nonparticipants in terms of ethnicity, sex, poverty status, and academic risk (see Table F.8 for complete frequency distributions of these variables). While the differences shown are not dramatic (the greatest being 10 percentage points) they are all statistically significant. Moreover, there are some interesting implications in the direction of these differences. The percentages given for the former UB participants who enter PSE closely reflect the percentages for the full UB participant group. The percentages of ethnic minorities, males,

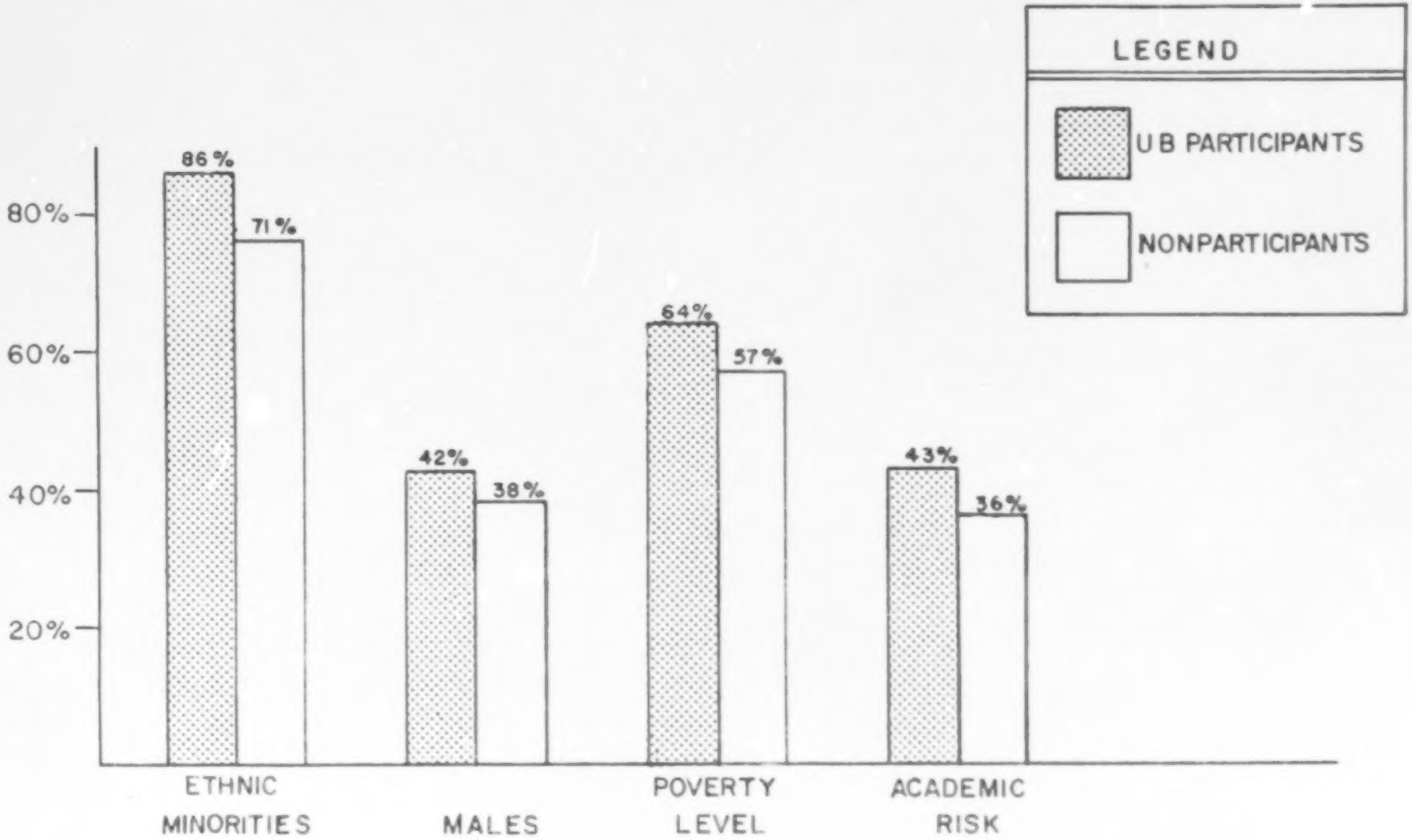

Figure 4.8. Some demographic and background differences between the subgroups of former Upward Bound participants and nonparticipants who entered postsecondary education (all grade cohorts)

poverty level individuals, and academic risks in the total UB participant population are estimated as 82 percent, 44 percent, 64 percent, and 47 percent, respectively (see Table F.40). As seen from Figure 4.8, the analogous estimated percentages within the PSE-entering UB population are quite similar. Thus, within the UB participant group, entry into PSE is not particularly restricted by these factors (except possibly in interaction).

Within the comparison group of nonparticipants, the situation is somewhat different. The estimated percentages of ethnic minorities, males, poverty level individuals, and academic risks within total adjusted comparison population are the same as those for the total UB participant population because statistical adjustment was performed for precisely this set of variables. Within the subgroup entering PSE, however, the estimated percentages are consistently and significantly less for the nonparticipant group.[14] Thus, for nonparticipants, PSE entry is differentially selective as related to the factors under consideration.

While the entire matrix of the various conditional probabilities involved in these considerations was not produced or tested for statistical significance, the implications from the data presented in Figure 4.8 are relatively straightforward. PSE entry is differentially partitioning the nonparticipant group in regards to ethnicity, poverty-level status, risk status and sex, but PSE entry does not differentially partition the UB participant group. Stated another way, albeit less precisely, PSE entry within the UB participant group is equally likely regardless of sex, race, financial means, or academic risk; however, such is not the case within the nonparticipant group.

B. Characteristics of Postsecondary Institutions Entered

This subsection examines the characteristics of those PSE institutions last or currently attended (and presumably those initially entered in many cases) by former UB participants and nonparticipants. The institutional characteristics considered in this subsection are indices of student body family income, selectivity, cost, size, proportion of student body receiving federal aid, average per student financial aid, predominant ethnicity of student body, institutional control, level of offering, and institutional host status for both UB projects and Special Services for Disadvantaged Students (SSDS) projects.[15] The indices were computed from data obtained from external sources, as described fully in Appendix D. These results are necessarily based on the subset of individuals who indicated PSE entry.

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[14] Some percentages would, of course, be even lower in the unadjusted comparison group, since adjustments compensated for the lower percentages of ethnic minorities, academic risks, and poverty level individuals within the unadjusted comparison population. See Table F.40 in Appendix F for additional detail regarding the unadjusted comparison population.

The analytic approach to this investigation involved categorizing sample members by type of institution attended and then using sample weights to estimate the population distribution of PSE-entering students among the various types of institutions. A comparison of differences in the distribution for former prticipants and that for nonparticipants was then used to examine differential institutional attendance patterns. This procedure was employed separately for each of the specific indices considered, and all such comparisons were performed only for the aggregate participant and nonparticipant groups, since there were no theoretical reasons to suggest grade cohort differences. Both the approach to analysis and the quality of data from the external file (see Appendix D, Section VII) imposed some limitations on these findings, which will be explored briefly.

Among the unique postsecondary institutions attended by the sample members, there were relatively large numbers of institutions that could not be classified, since specific indices could not be determined due to data indeterminancy in external file from which they were constructed. For all but two of the institutional indices considered[16] percentages of individuals attending these unclassifiable institutions were large, ranging from a low of 11 percent to a high of 36 percent among former participants and from 18 to 43 percent among nonparticipants. Techniques used to adjust for indeterminate data in other analysis were not employed here, since it did not seem prudent to adjust for institutional factors on the basis of individual background similarities. Further, attempts at imputation for this kind of indeterminacy would also have been highly speculative, and were therefore not attempted.

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[15] The SSDS program is an educational intervention program for college students; this program, together with the UB and Talent Search programs constitute the so-called TRIO programs.

[16] The exceptions were the indices of UB and SSDS host status. These indices had been obtained from information provided by USOE and could be defined for any PSE institution attended.

Instead, the indeterminate classification of an institution on a particular index was used as a separate reporting category. This poses particularly difficult problems of interpreting group differences on determinate values of the index when there are also group differences for the indeterminate classification. In a way, however, the PSE institutions comprising the unclassifiable category can be thought of as separate class of institutions. Many of the institution for which classifying information were not available were small proprietary schools, which may be considered similar in respect to certain institutional characteristics; the majority of such schools are probably low in selectivity and cost, offer two years or less, and receive little direct federal aid. If one can believe that the schools making up the unclassifiable category are somehow similar in this respect then some insight may be gained into what such indeterminate classification represents for a particular index.

Another limitation is imposed by the fact that the institutional indices are analyzed separately. Such analyses are not independent due to the fact that institutional indices are related, and thus the statistical level of significance for the several tests are interdependent and, taken severally, underestimate the likelihood of chance differences. Nonetheless, the approach taken was considered the most meaningful way in which differential matriculation patterns could be examined.

To aid in interpreting these results, the relationship of the various indices for the 676 unique PSE institutions attended by sample members were quantified. Measures of association between postsecondary institutional indices varied from .11 to .64, with many between .40 and .60; the increase in predictability of one PSE index by knowing the value of another index varied from .00 to .33, with several values over .20.[17] As an example, student body family income at an institution is related to average cost per student and to the proportion of students receiving federal aid; institutions with low student body family income are typically also those with low per-student costs and with greater percentages of students receiving federal aid. Any observed difference between the attendance rates of participants and nonparticipants in regards to the student body family income at an institution would therefore be at least partially reflected in differences related to institutional costs or federal aid availability.

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[17] Categorical statistical approaches were used in producing these values. The measure of association used was Cramer's Statistic (or the Phi Coefficient for two by two contingency tables). The symmetric Goodman-Kruskal Lambda was used to measure increases in predictability. While the full pattern of associations between the institutional indices is not reported here, it is available in Table F.19 of Appendix F.

1. Student Body Family Income

Four classifications were used for the index reflecting family income of students enrolled at an institution--unclassifiable, low, middle, and high. The index values reflected relative, rather than absolute, categories of student body family income (see Appendix D, Section V, for greater detail). The only significant differences between the attendance patterns of former UB participants and nonparticipants over the different types of institutions defined by this index involved the categories unclassifiable and low. Over one-third of the nonparticipants (37 percent) attended institutions that were unclassifiable, as compared to 22 percent of the participants. A reversal of this difference was found for institutions classified as having low student body family incomes; 34 percent of the former participants attended such institutions as compared to 19 percent of the nonparticipants.

If one accepts the assumption advanced above that most of the unclassifiable schools have students with low family income, then there are no differences in the distribution of the two groups over institutions in respect to this index. Approximately 27 and 17 percent of both groups attended schools classified as having middle or high student body family incomes, respectively (see Table F.20 for details).

2. Institutional Selectivity

Five classifications were used for this index--unclassifiable, low, moderately low, moderately high, and high. This index was also computed as a relative rather than an absolute measure, and was based on average scores of the student body on certain standardized tests (see Appendix D, Section V, for detail). In both groups approximately 9 percent had attended the high selectivity schools; 15 percent had attended moderately high selectivity schools; and 12 percent had attended moderately low selectivity schools. Significantly differential attendance patterns were observed only within low selectivity and unclassifiable institutions. Greater percentages of former participants (30 percent) attended low selectivity schools than nonparticipants (19 percent), and greater proportions of nonparticipants (43 percent) attended unclassifiable schools than did participants (36 percent). Assuming that the unclassifiable schools are also low selectivity schools implies no differential institutional attendance patterns between former participants and nonparticipants in regards to selectivity (see Table F.21 for details).

3. Average Per-Student Cost

Six classifications were used for this index--unclassifiable, less than $2,000, $2,000 to $2,499, $2,500 to $2,999, $3,000 to $3,999, and $4,000 or more. With the exception of the significantly greater percentages of nonparticipants (38 percent as compared to 23 percent of former participants) attending the unclassifiable institutions, there was only one marginally significant difference between the attendance distributions of the two groups in respect to institutional cost. Considering the size of that difference (four percentage points) and the interdependence of the various tests, this marginal difference was dismissed. Assuming that the costs at unclassifiable institutions were relatively low (e.g., mostly in the less than $2,500 categories), approximately 65 percent of both groups attended schools at which costs were less than $2,500; 28 percent attended schools with per-student costs between $2,500 and $3,999; and the remaining 7 percent attended schools costing $4,000 or more (see Table F.20 for details).

4. Institutional Size

Six classifications were used for this index of number of FTE students at the school--unclassifiable, less than 500, 500 to 999, 1,000 to 2,999, 3,000 to 7,999, and 8,000 or more. In terms of this breakdown of size, the only category within which significantly different proportions of former participants and nonparticipants had attended was the unclassifiable category. Assuming these schools fall in the smaller size categories, the distributions of both groups over institutions, according to size were approximately as follows: less than 1,000 students, 36 percent; 1,000 to 2,999 students, 28 percent; 3,000 to 7,999 students, 19 percent; and over 8,000 students, 17 percent (see Table F.23 for details).

5. Percentage of Student Body Receiving Federal Aid

Four classifications were used for this institutional index--unclassifiable; low aid, less than 15 percent of students receiving federal aid; moderate aid, 15 to 29 percent of students receiving aid; and high aid, 30 percent or more of students receiving aid. The distribution of former UB participants over these four institutional types was fairly uniform: 21 percent, 25 percent, 25 percent, and 29 percent, respectively. The analogous distribution for the nonparticipants was 36 percent, 25 percent, 19 percent, and 20 percent.

The unclassifiable institutions again tend to cloud the picture, but, if one is willing to assume that the unclassifiable institutions represent schools with low aid, then there is a suggested tendency of differential attendance at the low and high aid schools. Former participants attend the high aid schools in statistically greater relative numbers and the low aid schools in statistically lower relative numbers as compared to nonparticipants (see Table F.24 for details).

6. Average Per-Student Aid

Five classifications were used for this institutional index--unclassifiable; low aid, less than $50 per student; moderately low aid, $50 to $99 per student; moderately high aid, $100 to $899; and high aid, $900 or more. The distribution of former UB participants over these five types of institutions was 21 percent, 11 percent, 15 percent, 47 percent, and 6 percent, respectively. The analogous distribution of nonparticipants was 30 percent, 11 percent, 16 percent, 35 percent, and 8 percent.

The percentages of the two groups attending the various types of institutions differed significantly only for the unclassifiable and moderately high categories, greater percentages of nonparticipants in the former and greater percentages of participants in the latter. Again assuming that the unclassifiable category consists of mostly low aid schools, there is an additional, but related, suggestion from these results that former UB participants attend the higher aid schools in greater relative numbers than nonparticipants (see Table F.25 for details).

7. Predominant Ethnicity of Student Body

Three classifications were used for this index--unclassifiable, Black, and white. Similar percentages of former UB participants and nonparticipants had attended institutions classified as having predominantly white student bodies (70 percent and 68 percent, respectively). A significantly greater percentage of the former participants (15 percent) had attended institutions classified as being predominantly Black than had nonparticipants (8 percent); conversely, the unclassifiable schools had been attended by greater proportions of the nonparticipants (24 percent) as compared to participants (15 percent).

While it is difficult to theorize the racial composition of the schools with indeterminate classification, they were definitely not the traditionally Black institutions that comprised the predominantly Black student body classification. For this latter type of school, the proportion of former participants attending was nearly twice that of nonparticipants, although representing a small percentage of the total group in both cases. It is unlikely that this represents any racial preference difference between the groups, rather the difference is probably explainable by the relationship of this institutional index to the UB host status of the institution. As shown below in Subsection III.B.10, former participants attended UB host institutions in much greater relative numbers than nonparticipants, and greater proportions of the traditionally Black colleges serve as hosts for UB programs than predominantly white schools (see Table F.26 for details).

8. Institutional Control

Four classifications were used for this index--unclassifiable, public control, private control, and proprietary school. The majority of individuals in both groups attended public institutions (62 percent and 59 percent for participants and nonparticipants, respectively). Private institutions were attended by 24 percent of the former participants and 18 percent of the nonparticipants. Institutions classified as proprietary schools were attended by small percentages of individuals in both the former participant group (1 percent) and the nonparticipant group (2 percent); however, a significantly larger proportion of the nonparticipant group (21 percent) attended the unclassifiable schools (which do represent, in many cases, proprietary schools) than did former

UB participants (12 percent). This latter difference was the only statistically significant one, and suggests greater attendance of proprietary schools within the nonparticipant group (see Table F.27 for details).

9. Institutional Level of Offering

Four classifications were used for this index--unclassifiable, two-year (or less), four-year, and university. The distributions of former participants and nonparticipants over these types of institutions were markedly and statistically different, as shown in Figure 4.9; all attendance differences between former participants and nonparticipants are statistically significant, with the exception of the university category (which closely approaches statistical significance). About half of the nonparticipants attend the unclassifiable or two-year schools; the remainder attend the four-year colleges or universities (the unclassifiable schools are assumed, with some justification, to offer programs of two years or less). Almost two-thirds of the former UB participants, however, were in attendance at four-year colleges or universities (see Table F.28 for additional detail).

This finding is directionally consistent with that of the base-year study, which indicated that, among individuals in the twelfth-grade cohort who entered PSE immediately following their senior year, 76 percent of the former UB participants as compared to 45 percent of the nonparticipants entered four-year colleges or universities. In light of the heavy emphasis placed by the UB program on a four-year college education as opposed to a vocational program, the finding is quite reasonable. Moreover, the large majority of UB programs are hosted by four-year colleges and universities, and (as shown in the following subsection) former participants enter UB host institutions in greater relative numbers than nonparticipants.

10. Upward Bound and Special Services Host Status of Institution

These two institutional indices were relatively free of indeterminacy;[18] the results are therefore considerably less assumption-bound

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[18] The index was indeterminate for 9 nonparticipants and 22 former participants for whom the exact institution last attended could not be determined. There was no significant differences in the indeterminacy rate in the two groups, and for convenience they were considered to have entered nonhost institutions.

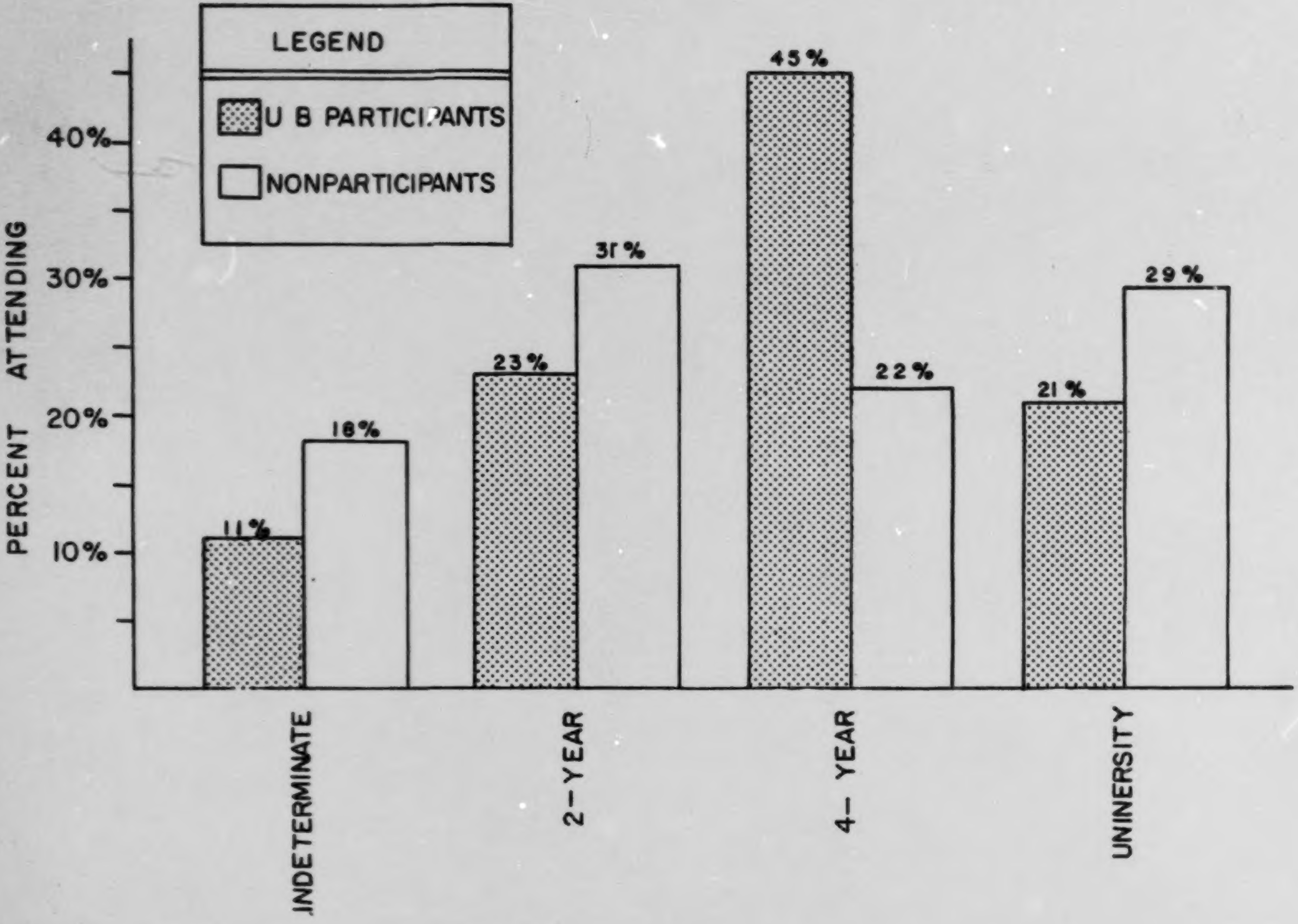

Figure 4.9. Distributions of former Upward Bound participants and nonparticipants among institutions with different levels of offering (all grade cohorts)

than previous ones. There were only two categories of institutions--those hosting UB (or SSDS) projects at any time during the last three academic years and those not hosting a project during that time. The proportions of former UB participants and nonparticipants attending host institutions, either UB or SSDS, are given in Figure 4.10.

As a group, almost half of the former UB participants had attended UB host institutions as compared to less than a fourth of the nonparticipant group. This difference, which is significant in both a statistical and practical sense, cannot be explained by attendance proximity since the nonparticipant sample was drawn from the same locations as the participant sample. The host institutions are supposedly committed to the program and to serving its participants, and these findings certainly suggest that enrollment of former participants in host institutions is disproportionately large relative both to the nonparticipant enrollment and to the proportion of host institutions in the institution population. It is expected that at least part of this difference is attributable to the fact that a fairly large number of UB participants are admitted to the particular institution that served as host for the specific UB project in which they participated.[19]

Figure 4.10 also indicates that the relative number of individuals entering host institutions increases directly with extent of former UB participation. The difference between one-year participants and greater-than-one-year participants was statistically significant; the difference between two-year and three-year participants was not (see Table F.29 for details).

Attendance patterns of former UB participants and nonparticipants at SSDS host institutions did not differ as dramatically as seen in Figure 4.10 (see also Table F.30 for details). Although the data indicates that former participants attended SSDS host institutions at somewhat higher rates, the difference is not a significant one. Within the participant group, it can be seen that three-year participants have attended SSDS host institutions at higher rates than one-year or two-year participants. The difference in attendance rate for two-year and

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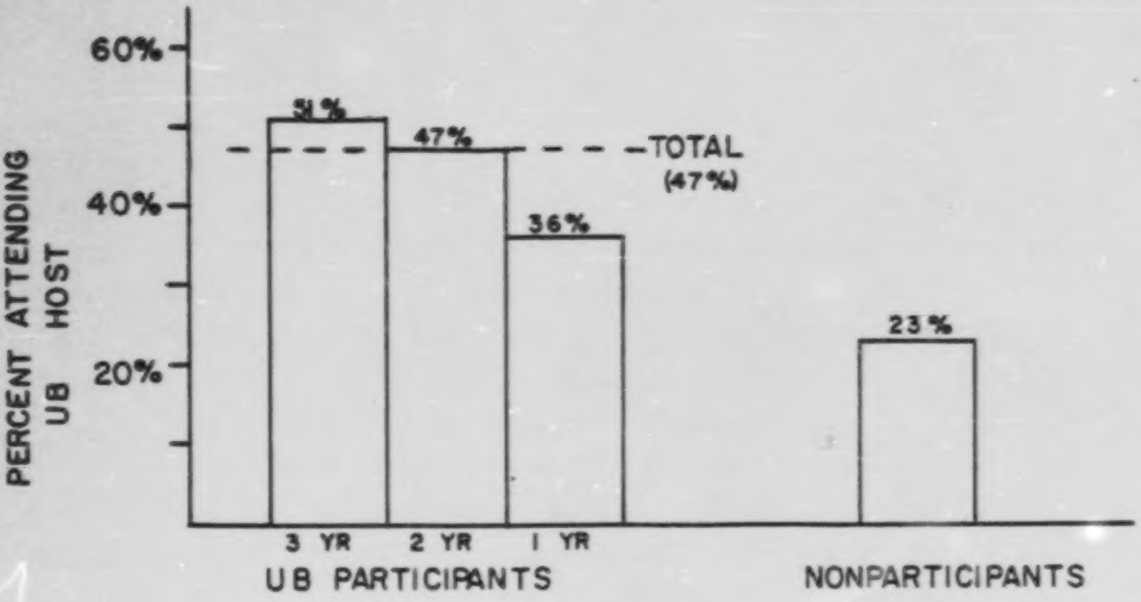
[19] During the base-year study, site-visit observations indicated that host institutions did enroll substantial numbers of the students served by the UB project on campus.



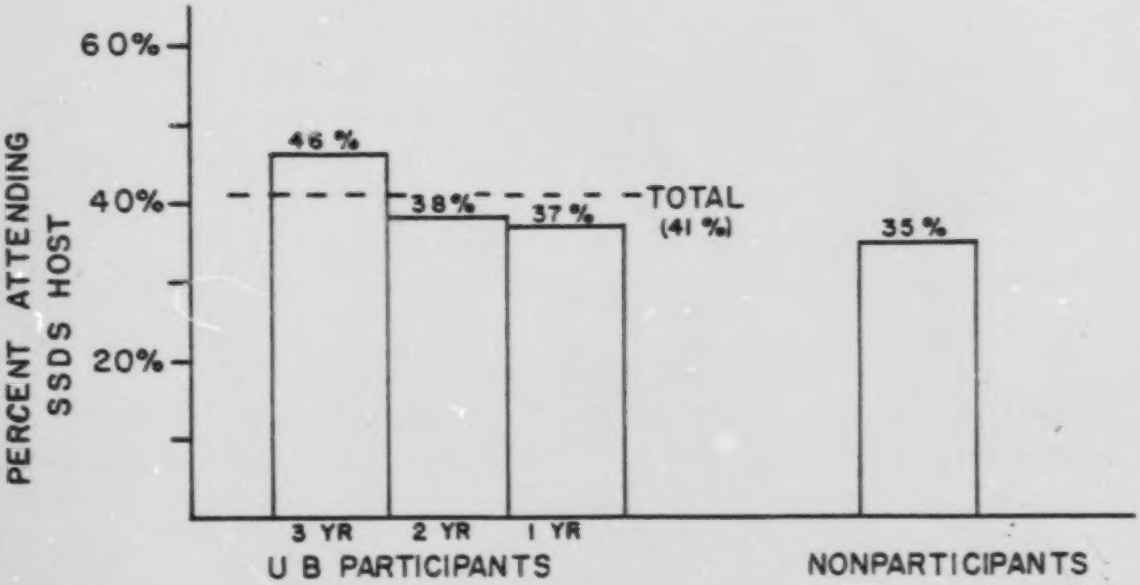


Figure 4.10. Percentages of former Upward Bound participants and nonparticipants attending institutions hosting Upward Bound or Special Services to Disadvantaged Students programs (all grade cohorts)

three-year participants is statistically significant; however, attendance rates for one-year participants do not differ significantly from those for greater-than-one-year participants.

IV. POSTSECONDARY PROGRESS AND PERSISTENCE

Having examined differences in the PSE-entering subgroups and differences in institutions attended, attention will now be directed to the important matter of the progress and persistence of former participants and nonparticipants who entered PSE. The legislatively mandated objective of the UB program is to provide participants with the "skills and motivations necessary for success in education beyond high school." The findings presented in this section do not attempt to address either the matter of which skills and motivations are necessary conditions for success in postsecondary education or the entire complex of defining postsecondary success. Rather, the findings focus on differences between the subgroups of former UB participants and nonparticipants who entered PSE in respect to two measures of intermediate progress/ persistence, highest grade level given PSE entry and current enrollment given PSE entry.[20] It should be kept in mind that both of these measures are timebound. Also considered within this section are possible moderations of any PSE progress or persistence differences as a function of the type of institutions attended and as a function of differential student perceptions of financial aid problems. In terms of the specific analysis questions posed in Chapter 1, this section addresses questions 1, 1.a, 1.b, and 3.

Two cautions, related to the findings in Section III, should be considered. It was shown that the former participants attended different kinds of schools than the nonparticipants. Former participants attended four-year institutions, predominantly Black institutions, and institutions that served as hosts for UB projects at greater rates than nonparticipants; there was also a suggestion that former participants attended schools offering more financial aid. The progress and persistence measures do not necessarily mean the same thing at these different types of institutions.

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[20] The first of these is more a measure of progress; the second measure attempts to operationalize persistence. It should be pointed out, however, that the particular aspect of persistence reflected in that measure is simple continuance and not any long term perseverance to an educational goal.

It was also shown in Section III that the subgroup of PSE-entering former participants differed from the subgroup of PSE-entering nonparticipants on certain background factors that are theoretically related to success, and that some statistical control has been lost as a result. This loss of statistical control is compounded when one further partitions the subgroups by type of institution attended or by perceptions of financial aid problems.

A. Average Highest Grade Completed and Current Enrollment, Given Postsecondary Entry

Comparisons involving average highest grade completed within the subgroup of individuals entering PSE generally showed no differences between former UB participants and nonparticipants either within-grade cohort or in the aggregate (see Table F.9 for details). In all but one instance, differences between the two groups were less than one-tenth of a grade level and did not approach statistical significance.[21] Comparisons were also made within the UB participant group entering PSE, both within grade cohort and in the aggregate. Only one of the eight contrasts showed a statistically significant difference and the difference involved was less than one-tenth of a grade level.[22]

Since the absolute magnitude of statistically significant differences was quite small and since differences were in no way systematic, it was concluded that no general relationship existing between UB participation and postsecondary educational progress was evidenced in these data. On the other hand, given the fact that UB participants entering PSE are qualitatively different from PSE-entering nonparticipants on factors known to be related to PSE progress, there is no evidence that they are progressing at any slower pace.

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[21] The one exception occurred within the eleventh-grade cohort, in which former participants reported an average grade completed that was fifteen-hundredths greater than that reported by the nonparticipants, which reached statistical significance.

[22] The specific difference referenced was that between two-year and three-year participants after aggregating over grade cohort.

Differences in the current enrollment percentages of former UB participants and nonparticipants who entered some form of PSE since 1974 were also far from clearcut. Current enrollment rates as a function of UB participation are given in Figure 4.11 (see Table F.10 for greater detail). From the presentation it can be seen that nonparticipants were continuing in PSE at equal or higher rates than former UB participants within every grade cohort. Within the twelfth-grade cohort, the difference in current enrollment between former participants and nonparticipants was 12 percentage points, which was the only statistically significant difference.

Examining current enrollment over grade cohort, one observes a sharp drop in current enrollment for both groups from the tenth-grade cohort (who, under normal progression, would have first entered PSE during the year of the follow-up study) to the eleventh-grade cohort; differences between the eleventh- and twelfth-grade cohorts are less dramatic. Additionally, Figure 4.11 indicates that the discrepancy in current enrollment between former participants and nonparticipants is directly related to high school grade in 1973. This suggests a relationship between nonparticipant-participant differences and length of time since initial enrollment in PSE.[23]

From Figure 4.11 it can be seen that differences were generally small among UB participants with different amounts of exposure to the program, except within the eleventh-grade cohort; the only statistically significant contrast was between one-year and greater-than-one-year participants in that cohort. There is, therefore, little indication that length of program participation is related to educational persistence as measured here.

The difference between the total participant group and the nonparticipants in the twelfth-grade cohort is difficult to interpret. There are many reasons why former UB participants may not be enrolled at as high rates as nonparticipants for the time point considered. Some of these reasons are related to the type of institution attended or to financial aid problems (these possibilities are examined in the following subsections). Other reasons may relate to timing of educational progress

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[23] The relationship suggested is somewhat clouded by previously demonstrated differences between the two groups in lag entry into PSE.

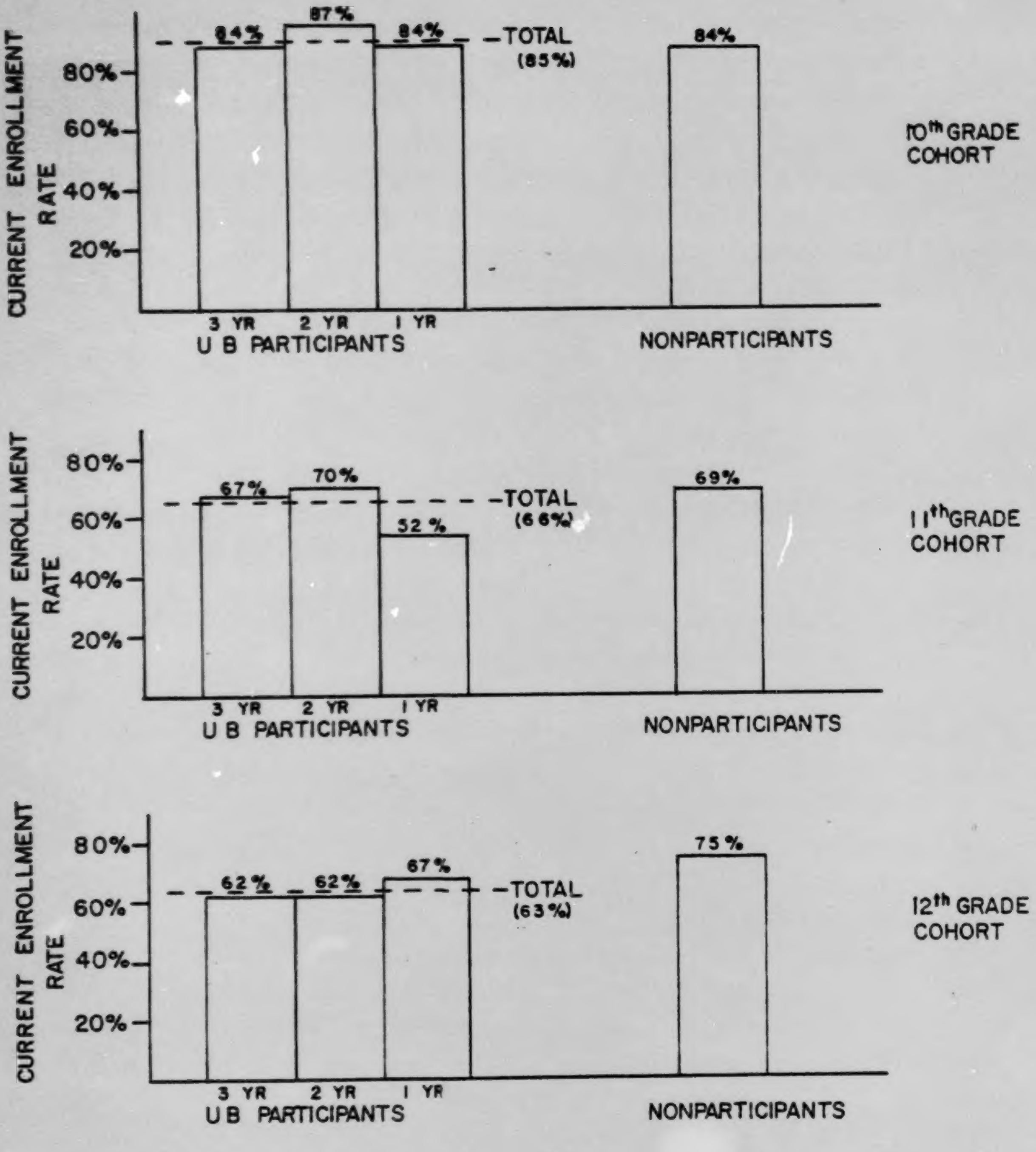

Figure 4.11. Current enrollment rates of former Upward Bound participants and nonparticipants who entered postsecondary education, by grade cohort

(see Subsection I.A above) or to differences between the PSE-entering subgroups.

B. Postsecondary Progress and Persistence as a Function of Characteristics of Institutions Attended

It is reasonable to expect that the postsecondary progress and persistence of individuals would vary as a function of the type of PSE institution they attend. It is also possible that differences in postsecondary progress and persistence between former UB participants and nonparticipants might be modified when one considers the type of institution attended. This subsection explores these possibilities using the previously defined variables, highest grade given PSE entry and current enrollment given PSE entry. The institutional characteristics considered are institutional level of offering, UB host status, and SSDS host status.

1. Institutional Level of Offering

The level of offering of an institution places obvious limits on the amount of postsecondary progress that is possible. PSE progress and persistence differences between UB participants and nonparticipants were examined as a function of this institutional characteristic (the analyses are summarized in Tables F.11.1, F.11.2, F.12.1, and F.12.2). The four categories of institutional offering considered were: (a) indeterminate classification,[24] (b) two-year, (c) four-year, and (d) university. The use of four categories precluded rigorous analysis within-grade cohort and only the aggregate groups were examined statistically. Although cell sizes were typcially too small for stable estimates, the estimates were computed within grade cohort; standard errors for such estimates were not computed, however.

In regards to postsecondary educational progress, as measured by average highest grade completed, the results were not particularly revealing. Average progress was greater for both participants and nonparticipants in the four-year colleges or universities than at two-year or unclassifiable schools, as expected. Former participants did not differ from nonparticipants in terms of postsecondary progress regardless of the institutional level of offering. Further no systematic differences were found in educational progress for length of UB participation as a function of institutional offering.[25] While no statistical tests were performed within grade cohorts, former participants and nonparticipant differences in postsecondary progress revealed no obvious systematic relationship to level of offering within any cohort.

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[24] Many of the institutions attended by individuals were not defined in terms of highest offering. Since these institutions were suspected to be proprietary schools, the indeterminate classification schools were used as a separate analysis category.

The examination of current enrollment rates as a function of institutional level of offering revealed that current enrollment rates were greater at four-year institutions and universities than at unclassifiable or two-year institutions, as might be expected. Differences in enrollment rates between former UB participants and nonparticipants were also related to level of offering. Specifically, while the two groups did not differ in enrollment rates at the unclassifiable or four-year institutions, nonparticipants were enrolled at significantly higher rates at two-year institutions (56 percent as compared to 67 percent for former participants) and at universities (94 percent as compared to 77 percent for former participants). There were no significantly differential persistence rate patterns within the participant group as a function of institutional offering.

Given the finding for the aggregate group, it is natural to examine possible differences within grade cohorts. Although no statistical significance is placed on such examinations, they are considered of sufficient descriptive interest to present. At the four-year or unclassifiable institutions, only one difference in current enrollment rates between former participants and nonparticipants was greater than four percentage points (none were greater than 9 percentage points) within any grade cohort. At two-year institutions, differences between the two groups were relatively small for the tenth- and eleventh-grade cohorts, but within the twelfth-grade cohort 67 percent of the nonparticipants were still enrolled as compared to 46 percent of former participants. One of many possible reasons for this pattern of differences in current enrollment is that former participants completed the two-year course of study in two years.

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[25] One marginally significant difference was found. Three-year participants at four-year institutions showed twelve-hundredths of a grade level less progress than two-year participants at the same type of institutions.

The pattern of differential current enrollment over grade cohort within universities exhibited an unexpected structure. Within the tenth-grade cohort (for, presumably, the first year in PSE) large percentages of both groups were still enrolled (88 percent of the former participants and 84 percent of the nonparticipants). High current enrollment was also observed for the eleventh-grade cohort, but differential continuance between the groups had widened (83 percent of former participants and 94 percent of nonparticipants were still enrolled). Within the twelfth grade cohort, 71 percent of the former participants were still enrolled while 95 percent of the nonparticipants were still enrolled. As can be seen from the values presented, the percentage still enrolled at universities was relatively constant or increasing over grade cohorts for the nonparticipants but was decreasing over grade cohorts for former UB participants. Since no statistical significance is attached to the figures due to small cell sizes, interpretation of these findings is not attempted.

2. Upward Bound Host Status

Another institutional characteristic which in theory may influence PSE progress and persistence of individuals is whether or not the institution serves as a host for a UB project. Host institutions are committed to the UB program and to serving low-income and ethnic minority youth. One might expect equal or greater progress and persistence among those individuals attending these institutions; further one might expect some differential progress and persistence of former UB participants as compared to nonparticipants at host institutions. The analyses performed to examine these suppositions were performed both within grade cohort and in the aggregate; however, no contrasts were made within the UB participant group (see Tables F.13 and F.14 for details).

In regards to average highest grade completed, both participants and nonparticipants demonstrated, in the aggregate, slightly greater progress (one-tenth to two-tenths of a grade level) at the UB host institutions. While there was no difference between the progress of participants and nonparticipants at nonhost institutions, former participants reported slightly, but not significantly, greater progress at host institutions (less than two-tenths of a grade level). This pattern was only replicated within the eleventh-grade cohort and was therefore not considered to be of major practical significance.

In regards to current enrollment, the results suggest that, in the aggregate, current enrollment rates for former participants were slightly, but not signigicantly, less than those for nonparticipants at nonhost institutions (66 percent and 71 percent, respectively). At host institutions, however, enrollment rates for former participants (68 percent) were significantly less than those for nonparticipants (84 percent). This same pattern was replicated within both the eleventh- and twelfth-grade cohorts. The replicability of the finding in the two grade cohorts (particularly within the two cohorts that would have been in PSE for over a year with normal progression) suggests that the finding is not an artifact of aggregation. The finding resists an easy interpretation.

3. Special Services Host Status

The SSDS program is designed to assist educationally disadvantaged college students enrolled at the host institution. Further, information gained during base-year site visits suggested that some UB projects specifically attempt to place their students in institutions with SSDS projects so that they could benefit from that program. For these reasons and others, expectations of progress and persistence differences as a function of SSDS host status are similar to those advanced for UB host status. The analyses performed to empirically test these expectations were completely analogous to those of the previous subsection (see Tables F.15 and F.16 for details).

In the aggregate, educational progress (as measured by average highest grade completed) of former UB participants and nonparticipants did not differ at nonhost institutions but was statistically greater among former participants at host institutions. This pattern did not, however, replicate significantly in any of the individual grade cohorts.[26] Since the significant aggregate difference involved was less than two-tenths of a grade level and since the finding was not replicated within grade cohorts, this result was not considered to be of major practical significance.

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[26] In the eleventh-grade cohort former participants reported significantly greater progress at both types of institutions.

For the aggregate groups, educational persistence (as measured by current enrollment rates) was significantly greater for nonparticipants than for former participants at both SSDS host institution (78 percent versus 70 percent) and at nonhost institutions (73 percent versus 64 percent). From these values it can also be seen that both groups showed higher current enrollment rates at SSDS host institutions. This pattern replicated only in the twelfth grade cohort, which, it will be recalled, was the only grade cohort in which current enrollment rate differences were significantly greater for nonparticipants (Subsection IV.A above).

C. Postsecondary Progress and Persistence as a Function of Perceived Inadequacy of Financial Aid

Another factor that might be expected to relate to postsecondary progress and persistence (particularly for inidividuals of meager means) is the availability and adequacy of financial aid. One of the questions asked sample members in the Follow-Up Questionnaire was whether or not inadequate financial aid had been a problem in their obtaining the kind of education that they desired. The two measures of postsecondary progress and persistence were examined within the two subgroups of individuals defined by responses to that questionnaire item (i.e., those who reported inadequate financial aid as a problem and those who did not). Groups were considered in the aggregate rather than within grade cohort due to the small base rates of those reporting financial aid as a problem (see Section V). The analyses are summarized in Tables F.17 and F.18.

In general, postsecondary progress showed little relationship to perception of adequacy of financial aid as a problem. Within both the former participant group and the nonparticipant group, differences in highest grade completed were less than one-tenth of a grade level between those reporting a problem of inadequate aid and those not reporting it as a problem. Former participants did show marginally greater progress than nonparticipants within the subgroup reporting problems with financial aid. Further, no systematic or meaningful pattern of significant differences were observed when contrasting participants with differing amounts of program exposure.

Current enrollment differences for the aggregate groups initially seemed counterintuitive. Within both the former participant group and the nonparticipant group the subgroup reporting that financial aid had been a problem were currently enrolled at greater rates than were those who reported no financial aid problems. When one considers that the questionnaire item was posed relative to obtaining a *desired* amount of education, the observed direction of this difference could be explained by the fact that some of those who had not seen inadequate financial aid as a problem had already completed their desired level of education.

In the aggregate, nonparticipants were currently enrolled at greater rates regardless of whether they reported inadequate financial aid as having been a problem (80 percent as compared to 68 percent of the former participants) or not (72 percent as compared to 67 percent of the former participants). The difference was statistically significant only within the subgroup reporting financial aid as a problem. Although within grade analyses were not performed, it is likely that the differences reported here were primarily attributable to the twelfth grade cohort (see above, Subsection IV.A). *There were no statistically significant differences among former participants as a function of length of program participation within either the problem or the no-problem subgroup.*

V. OTHER STUDENT OUTCOMES

This section addresses the four secondary analysis questions listed in Chapter 1. These questions involve possible differences between former UB participants and nonparticipants in terms of such outcomes as work status, military service status, educational expectations and problems encountered in obtaining their desired level of education. Analyses reported in this section were typically performed for the entire group of individuals for whom data were available, and thus are not restricted to response-defined subpopulations. The large majority of the analyses were performed only for the aggregate (all grade cohorts) groups.

A. *Employment Status*

In considering employment status, differences between former UB participants and nonparticipants were examined for two mutually exclusive subgroups: those currently not enrolled in school and those currently enrolled in school either full- or part-time. This partitioning was undertaken because it had been anticipated that any differences between participants and nonparticipants in work status could be more meaningfully interpreted, given knowledge of their current enrollment status.[27] Very little in the way of meaningful relationships were revealed from these comparisons.

For those not enrolled in school, there were no significant differences between UB participants and nonparticipants in terms of the three employment categories (unemployed, working part-time, and working full-time) although the majority of both groups (58 percent of the nonparticipants and 52 percent of the former participants) were working full-time. There were also no significant relationships between extent of UB participation and work status, except in the case of part-time employment. For that employment category, significantly fewer one-year participants (5 percent) were working part-time than greater-than-one-year participants (10 percent). It is difficult to provide a meaningful interpretation to such a finding, given no other evidence of systematic differences in employment status (see Table F.31.1 for details).

In regards to those UB participants and nonparticipants who were enrolled in school, results were equally sterile. Over half of both groups were not working (60 percent of the former participants and 57 percent of the nonparticipants). While fewer of the enrolled former participants were working, percentage differences between participants and nonparticipants were typically small and not statistically significant. Within the participant group, one contrast reached statistical significance: more one-year participants reported working full-time than did greater-than-one-year participants. Again, in the absence of systematic differences within other work status categories, the finding does not lend itself to interpretation (see Table F.31.2 for details).

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[27] As an example, the state of not working full-time among the currently enrolled subgroup might reflect a better financial aid package but among those not currently enrolled it might reflect the inability to obtain a job.

B. Military Status

Examination of the differences between former participants and nonparticipants in terms of their current military status also failed to reveal any meaningful relationships. The great majority of both groups reported that they had never been in the military service. While a significant difference was found between the percentages of former UB participants and nonparticipants who were "never in service" (83 percent and 87 percent, respectively), this difference could be explained by the significantly greater incidence of indeterminate responses within the former participant group. Thus, UB participation appears to have little relationship to entrance into military service (see Table F.32 for details).

C. Educational Expectations

One of the aims of the UB program is to motivate its participants to seek and complete a postsecondary education. Given the successful attainment of this goal, one would expect significantly higher educational expectations among former UB participants than among nonparticipants. As can be seen in Figure 4.12, this is generally the case, though the differential rate of indeterminate data makes certain conclusions somewhat tenuous (see also Table F.33).

A significantly higher proportion of nonparticipants than participants expected to complete high school only. The same pattern holds regarding the expectation of successful completion of vocational, technical, or business school programs; statistically greater percentages of nonparticipants expect to complete their education within such noncollegiate programs than do former UB participants. While there is no significant difference between percentages of participants and nonparticipants expecting to complete a four-year college program, former UB participants do expect to complete a graduate program in significantly greater relative numbers (at double the rate for nonparticipants).

These findings are quite comparable to those reported in the base-year study, with two exceptions. Base-year results indicated significantly greater percentages of UB participants than nonparticipants expecting to finish a four-year program but no differences between the two groups in expectations regarding graduate school. Other results of

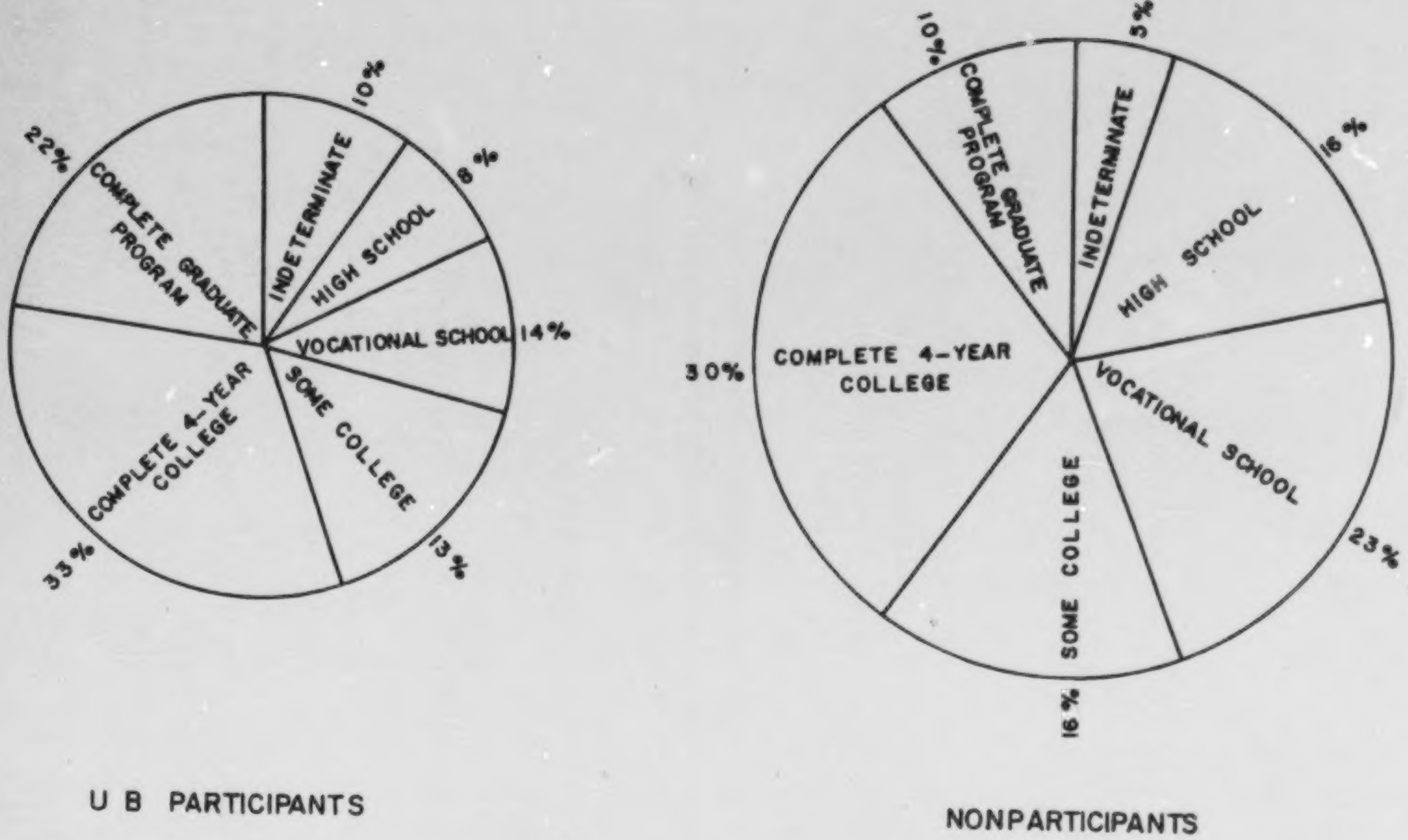

Figure 4.12. Highest education expected by former Upward Bound participants and nonparticipants (all grade cohorts)

these analyses not presented here (see, however, Table F.33) indicated generally higher educational expectations within the participant group as a function of length of program participation; however, these results were also somewhat attenuated by significantly differential rates of data indeterminacy among the various extent-of-exposure groups.

D. Educational Problems Experienced

The final set of reported analyses deals with a number of possible problems that an individual could have experienced in fulfilling his or her educational aspirations. A series of items in the follow-up questionnaire requested sample members to report whether they had encountered any of the following problems in obtaining their desired level of education: (a) poor grades, (b) poor high school preparation, (c) lack of family finances, (d) inadequate financial aid, (e) program unavailability, or (f) family responsibilities. The analysis of responses to these questionnaire items involved the total former participant and nonparticipant groups rather than the PSE-entering subgroups, since it was surmised that some problems experienced may have precluded PSE entry or high school completion (see Tables F.34 through F.39 for details).

There are some interpretational problems related to these findings in light of other findings. The questionnaire item specifically linked the problem areas to obtaining ones desired level of education, and, as indicated above, UB participants have higher educational expectations than nonparticipants. Further, as determined during the base-year study, UB participants receive a considerable amount of counseling directed toward circumventing various problems they might encounter in pursuing their education. It thus seems reasonable that former UB students would be more subject to (by virtue of their greater expectations), aware of, and sensitive to any problems experienced in pursuing their educational goals and that they would thus report them in greater relative numbers than nonparticipants.

Results of analyses investigating the types of educational problems reported by former UB participants and nonparticipants are depicted in Figure 4.13. An examination of the figure reveals that for most of the specific problem areas former participants report having experienced the problem in greater relative numbers than nonparticipants. In three

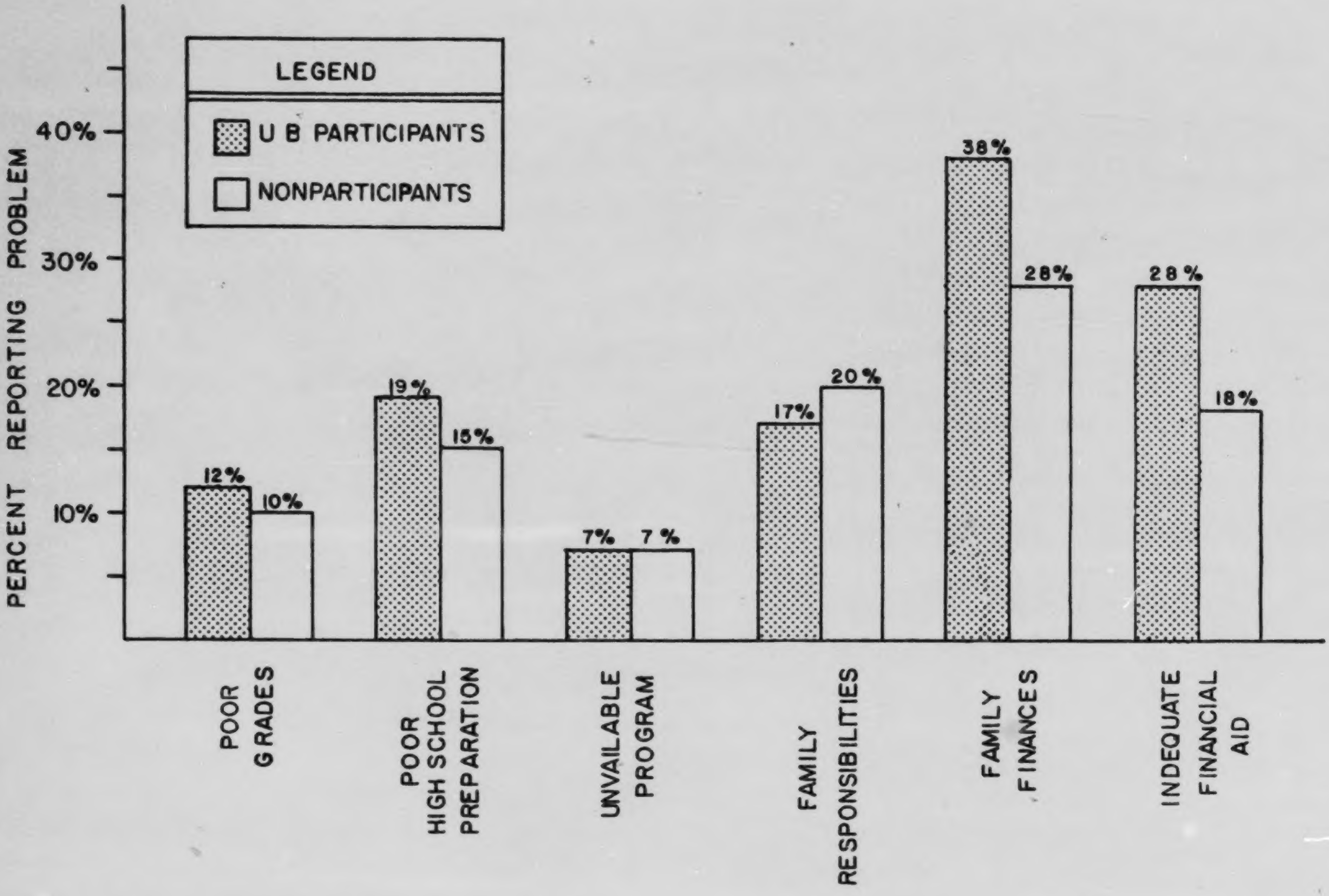

Figure 4.13. Percentages of former Upward Bound participants and nonparticipants reporting having experienced specific problems in obtaining their desired level of education (all grade cohorts)

areas--poor high school preparation, lack of family finances, and inadequate financial aid--statistically significant differences were found between percentages of former UB participants and nonparticipants reporting the area as a problem.[28]

Because of the policy relevance of the financial aid question, the analysis of reports of problems in this area was extended to include comparisons both within grade cohort and within the UB participant group. Results of these analyses are reported in Figure 4.14 (see also Table F.39). Within each of the grade cohorts, a similar pattern of results is evidenced (which was also reflected in the aggregate group). Greater percentages of former participants than nonparticipants reported inadequate financial aid as having been a problem in achieving their desired level of education, and the reporting of problems with inadequate financial aid is a direct function of length of program participation. The only significant within-cohort differences, however, were those between the total participant group and the nonparticipant group in grade cohorts eleven and twelve; however, the trend across the extent-of-participation groups was significant in the aggregate.

Another feature of Figure 4.14, which does not completely support a hypothesis of greater sensitization among former participants, may be seen by a comparison across the three grade cohorts. While the percentage of nonparticipants reporting problems of inadequate financial aid is relatively stable over cohorts, the analogous percentage of former participants is increasing (for the total participant group as well as for each of the extent-of-participation groups). Since there is no reason to expect that the UB participants are becoming more sensitized with the passage of time, this feature of the findings suggests that the UB participants may be actually experiencing more problems with financial aid than nonparticipants the longer they are out of high school. On the other hand, since it is fairly common to experience more financial problems the longer one remains in PSE, it could be that the former participants are simply better able to perceive such problems as a function of their sensitization.

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[28] Particularly in respect to the difference regarding financial aid, former UB participants may be more sensitized to what constitutes adequate financial assistance, since they typically receive more intensive financial aid counseling than the nonparticipants.

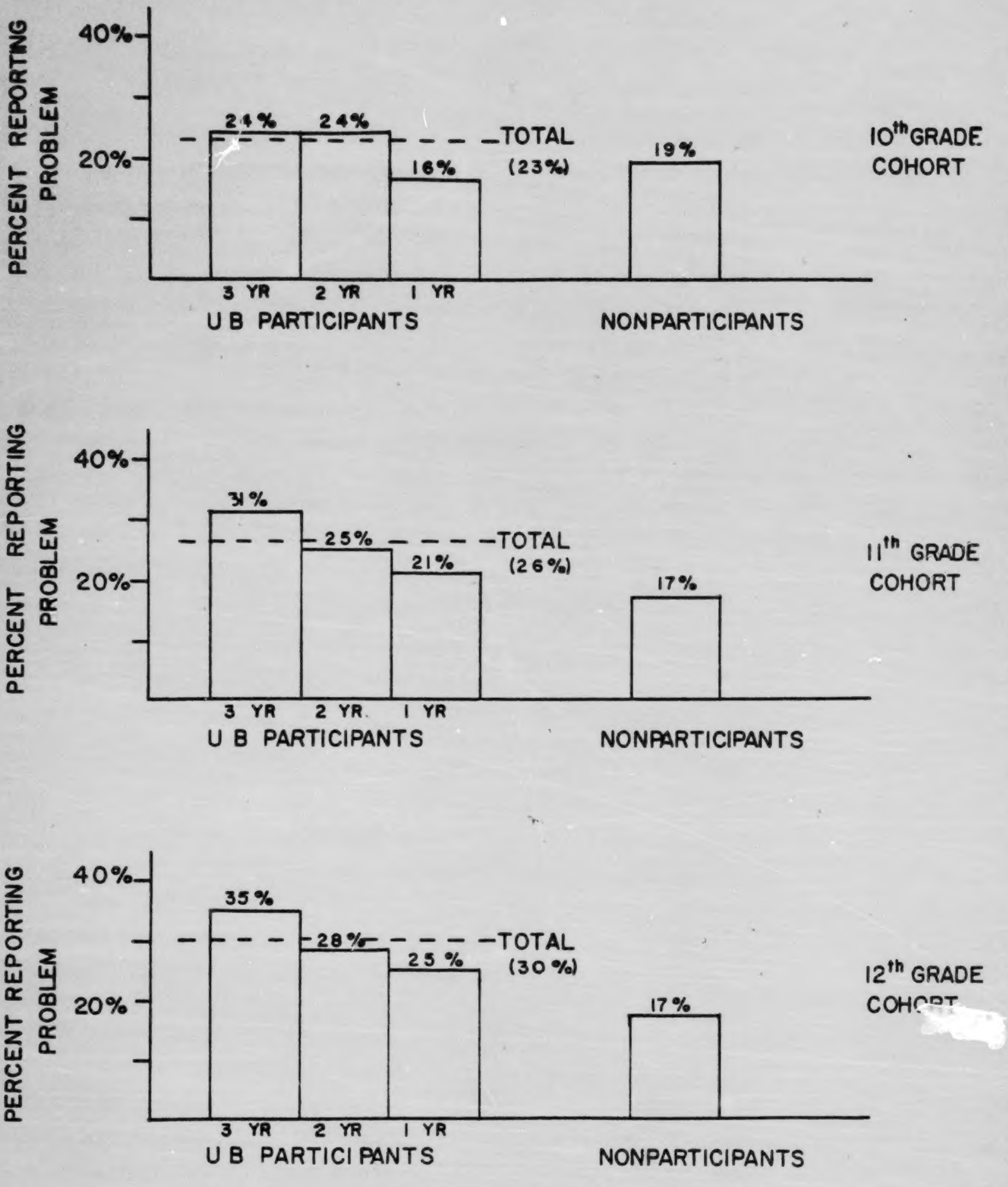

Figure 4.14. Percentages of former Upward Bound participants and nonparticipants reporting inadequate financial aid as a problem in obtaining their desired level of education, by grade cohort

VI. SUMMARY

This chapter has presented an investigation of the set of nine analysis questions that the follow-up study was intended to address. Although interpretation of the results presented are constrained by certain weaknesses in the measures and limitations of design and analysis, the major findings are, in general, internally consistent and in agreement with results of the base-year study. Some of the findings are of sufficient robustness to transcend the basic constraints, others are much more tentative.

The general educational progress and persistence of former UB participants is consistently greater than that for a statistically equated group of nonparticipant controls. The findings also indicate that general educational progress and persistence is positively related to length of participation in the UB program. The one step in educational progress and persistence that most obviously leads to superior general educational performance of former UB participants is entry into postsecondary education (PSE).

As a total group, former UB participants persist through completion of high school in large proportions. Approximately 93 percent of the former participant group had completed high school at the time of this study, although high school completion rates varied, as expected, as a function of high school grade during the base year (1973). A high rate of high school completion was also realized by nonparticipants (90 percent), who showed no significant difference from former UB participants in this regard. Although the positive relationship of high school completion to length of participation in the UB program is particularly suspect in some instances, results for the twelfth-grade cohort suggest that the relationship may not be completely artifactual. Findings related to high school completion rates generally agree with those of the base-year study.

Former UB participants persist through PSE entry, as a total group, in much larger relative numbers (70 percent) than do nonparticipants (52 percent). While PSE entry rates vary as a function of initial grade, the difference in PSE entry rate is stable over grade cohort. Although the positive relationship between unconditional PSE entry rate and length of program participation also reflects the attenuated relationship between high school completion and extent of participation, there is relatively strong evidence for such a relationship within the twelfth-grade cohort. Moreover, when one considers PSE entry rates among those who reported at least high school completion, the direct relationship between length of participation and PSE entry remains, both in the aggregate and within each grade cohort. Although some statistical control is sacrificed in this latter comparison, the conditional PSE entry rate is free of the attenuated relationship. Findings related to PSE entry are also directionally consistent with those of the base-year study.

The greater PSE-entry rate among former UB participants stems from two different entry patterns. As shown in the base-year results, former participants enter PSE in greater relative numbers immediately following high school completion. This finding was also reflected in the follow-up results within the tenth-grade cohort (who would have completed high school in the spring prior to the follow-up study, under normal progression). Additionally, the follow-up study indicates that among those who do not enter PSE immediately following high school, greater proportions of former participants (62 percent) than nonparticipants (36 percent) subsequently enter PSE.

The types of individuals entering PSE from the former participant group differ from those entering from the statistically equated group of nonparticipants. For UB participants, the proportions of ethnic minorities, males, poverty status individuals, and academic risks in the PSE-entering subgroup coincide with the analogous proportions in the total participant population. Among the PSE-entering subgroup of nonparticipants, all of these groups are underrepresented.

Additionally, the types of postsecondary schools attended by former participants are different from those attended by nonparticipants. Although the analyses addressing such differences were somewhat limited and assumption-bound both by strong relationships among the institutional characteristics considered and by the relatively large number of institutions that could not be classified, some meaningful findings did emerge. A greater percentage of former UB participants (66 percent) report current or former attendance at four-year institutions or universities than do nonparticipants (51 percent). This is consistent with the program emphasis and with results of the base-year study. Former participants also report greater attendance at institutions hosting an Upward Bound project (47 percent) than nonparticipants (23 percent). The data also suggest that UB participants attend the traditionally Black institutions at greater rates than nonparticipants (although this result is probably related to their greater attendance rates at UB host institutions) and that they attend institutions with more attractive aid policies at somewhat greater rates than nonparticipants.

Former UB participants also reported greater educational expectations than nonparticipants. While large proportions of both groups expected to complete at least a four-year postsecondary program, relatively fewer nonparticipants (40 percent) reported such expectations than former participants (55 percent). These results were basically in agreement with the base-year findings.

Former UB participants also reported having experienced more problems than nonparticipants in reaching their desired level of education. The three areas in which such problems had been differentially experienced by former participants were: poor high school grades, personal and family finances, and inadequate financial aid. In regards to inadequate financial aid, the problem seemed to be experienced at an increasing rate by former participants as a function of the length of time they had been out of high school (using grade cohort as a surrogate measure of time since high school completion). This finding is not particularly surprising in light of the fact that greater proportions of poverty status individuals and academic risks entered PSE from the former participant group. Further, the fact that educational expectations of former participants are greater may be reflected in the finding. Finally, it is possible that the extensive counseling of UB participants in areas of postsecondary survival has sensitized them to the extent that they view these problem areas within a different frame of reference.

The former UB participants and nonparticipants did not appear to differ in any systematic way with respect to outcomes that were not directly related to education. There were no differences in the proportions of former participants and nonparticipants who were currently or formerly in military service (about 11 percent in both groups) and there were no differences in current work status. The majority of former participants and nonparticipants who were not currently enrolled in school reported working full-time; the majority of both groups who were currently enrolled in school were otherwise unemployed.

The major investigative thrust of the follow-up study was directed toward educational progress and persistence among those former UB participants and nonparticipants who entered some form of postsecondary schooling. From the findings previously summarized, however, it is obvious that these investigations could not have provided completely clearcut results. The former participants entering PSE differed from PSE-entering nonparticipants. The educational expectations of the former participants were greater, they entered different kinds of postsecondary institutions, and they reported having experienced more problems in fulfilling their educational aspirations. Moreover, the measures of postsecondary progress and persistence available were particularly time-bound. In light of these limitations, the findings regarding postsecondary progress and persistence must be interpreted cautiously.

Analyses were unable to detect any consistent or systematic difference between former UB participants and nonparticipants in respect to postsecondary progress, as measured by highest grade completed. The one suggestion of a relationship between program participation and PSE progress was for the eleventh-grade cohort, in which former participants held a marginally significant advantage. This weak relationship was also reflected when considering the type of institution attended and whether or not problems with financial aid had been experienced. Former participants in the eleventh-grade cohort reported slightly greater progress than nonparticipants at UB host institutions, but no greater progress at institutions not hosting the program; they also reported slightly greater progress both at institutions hosting SSDS projects and at those not hosting such projects. In general, the UB participant group demonstrated slightly greater progress than nonparticipants within four-year colleges and within the subgroup of individuals reporting problems of inadequate financial aid.

Analyses were able to detect differences between former participants and nonparticipants in terms of current enrollment (a measure of postsecondary persistence reflecting simple continuance rather than long-term perseverance toward an educational goal). Nonparticipants who had entered PSE were currently enrolled at higher rates (76 percent) than were former participants (67 percent). This finding for the aggregate group was primarily attributable to a similar finding within the twelfth-grade cohort (the group farthest removed from high school).

This difference was further examined as a function of type of institution attended and whether or not financial aid problems had been reported. The results of these examinations indicated that the disparity between current enrollment of former participants and nonparticipants was concentrated within the universities, two-year schools, UB host institutions, and SSDS host institutions as well as among the subgroup of individuals reporting problems with adequacy of financial aid. The higher current enrollment rates of twelfth-grade cohort nonparticipants within two-year institutions has no particular significance, since 1973 twelfth-graders should have completed a two-year course of study by the time of the follow-up study. The other areas in which disparities exist provide a picture that resists an immediate or simple explanation, particularly in light of the findings regarding postsecondary progress.

While there are numerous alternate explanations possible for the findings regarding postsecondary progress and persistence, at least one exists that is consistent with the pattern of obtained results, if one is willing to accept grade cohort as a proxy for length of time in PSE. The former UB participants entering PSE are more needy than nonparticipants, but this seems to make little difference in either progress or persistence during the first year of postsecondary experience (tenth-grade cohort). By the second year of PSE (eleventh-grade cohort), the former participants are progressing equally well or slightly better than nonparticipants, but are beginning to feel more financial aid problems than nonparticipants. By the third year of PSE (twelfth-grade cohort) former participants have experienced even more financial aid problems and do not immediately re-enroll at as high a rate as do nonparticipants (particularly at the more expensive universities that host UB projects and/or SSDS projects).

This scenario is not presented as the only explanation of the findings, even though it is consistent with all results, but as one of many possible explanations, all of which would be little more than speculation in light of the complex of individual background and contextual differences involved. It is clear, however, that the three measures of postsecondary progress and persistence are quite time-bound and that they shed little light on any long-term postsecondary progress or persistence toward ultimate educational goals, for either the former UB participants or the nonparticipants. This study was not designed to address such long-term issues, which must be considered only after the passage of additional time.

Chapter 5

Student Outcomes as a Function of Background Characteristics, Upward Bound Participation, and Specific Project Association

I. OVERVIEW

While the comparative descriptive analyses reported in Chapter 4 addressed all of the specific analysis questions posed for the study, additional analyses were conducted to augment the results already presented. The two basic concerns impelling these supplementary analyses were: (a) the relationships of certain individual background factors, considered jointly with extent of former UB participation, to various outcome measures, and (b) the relationship of specific UB project association[1] to those outcome measures. The approach for this set of analyses was relational, using multiple regression analysis.

The specific issues addressed can be formulated into three general analysis questions, as follows:

a) To what extent and in what manner are individual characteristics (ethnicity, sex, age, poverty status, academic risk status), initial educational starting point (grade cohort), and extent of Upward Bound participation jointly related to educational expectations, persistence and progress?

b) To what extent and in what manner is context (as reflected in specific project association) related to educational expectations, persistence, and progress, when considered jointly with individual background factors, extent of Upward Bound participation, and initial educational starting point?

c) Are these relationships relatively stable within the various categories of individual classification? (i.e., Do the variables interact in their relationship to the educational outcomes considered?)

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[1] Although nonparticipants were not members of a UB project, by definition, they were associated with a project through their association with one of the high schools from which the UB project recruited and within which UB participants from that project were enrolled.

A nontechnical presentation and interpretation of the results of analyses addressing these questions is presented in subsequent sections of this chapter. Detailed technical information regarding the relational analyses is not presented but is available in the various appendixes of this report.[2] In particular, tabular summaries of all reported analyses are provided in Appendix G and references to appropriate tables in that appendix are provided with the presentation of results in this chapter. The remainder of this introductory section presents a summary of considerations basic to the analysis questions and the analytic approach (for greater detail, see Appendix G, Sections I through III).

A. Basic Investigative Rationale

The major thrust of these analyses was to examine relationships of various educational outcome measures to student background factors and specific project association as well as to extent of participation in the UB program, within a more flexible analysis framework than that of Chapter 4. The UB program does not represent a unitary approach to educational intervention (see Chapter 2); rather, there is considerable variation among projects in terms of the characteristics of students served, characteristics of staff members, particular strategies employed, and the context within which they operate. It is not unreasonable to expect that the educational outcomes of participants would vary as a function of some of these variables, taken either singly or in combination. Identifying different patterns of such variation would provide a more holistic framework within which the UB program could be viewed. The analyses presented in this chapter are therefore less directed toward the issues of national program impact addressed in Chapter 4 (for which variation of individual characteristics and contextual factors are incidental and should be controlled) than toward the interrelationships of the various factors thought to be associated with educational outcomes.

In considering these more inclusive questions, the choice of variables was an important one. Two basic types of variables were employed in the relational analyses: (1) those educational outcomes that were to be predicted (criterion variables), and (2) those factors that were theoretically and/or empirically related to educational outcomes and that were to be used in predicting them (predictor variables).

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[2] Appendix D provides the specifications for developing the analysis variables used; Appendix E provides a description of the various weight adjustment techniques; and Appendix G details the approach, rationale and limitations of the relational analyses.

The variables used to predict educational outcomes were, of course, limited to those available from the follow-up and base-year data base. Those considered were extent of UB participation (including no participation), specific UB project association, sex, age, ethnicity, poverty status, academic risk status, and initial high school grade level in 1973. Grade cohort, sex, and age are theoretically related to educational outcomes and were included for statistical control. Since the UB program is designed for students who traditionally have been excluded from higher education due to financial press, inadequate secondary preparation, or cultural or ethnic disequalization; poverty status, academic risk status, and ethnicity were included to determine empirically (1) the extent to which these variables increase or decrease the predicted outcomes, and (2) the extent to which UB participation compensates for any change in predicted outcomes attributable to those variables.

As suggested by the separate related analysis question, the inclusion of specific project association as a predictor variable warrants some elaboration. Taken singly, specific project association represents potential site differences, which may be expected to add to the prediction of educational outcomes. More importantly, any unique prediction of educational outcomes by specific project association in combination (interaction) with extent of UB participation suggests a variation over sites of participant-nonparticipant outcome differences within site and signals differential project effectiveness. The matter of differential project effectiveness was addressed by a similar set of analyses conducted during the base-year study.[3] Those analyses were unable to demonstrate any systematic or consistent unique relationship between project characteristics and the short-term student outcomes of that study when statistical corrections were made for existing differences between projects in characteristics of participants (see Chapter II, Section IV). It was anticipated that the more powerful[4] analyses of the follow-up study would reveal some unique contribution of differential project effectiveness in predicting the intermediate student outcomes of the current study.

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[3] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976, pp. 8.26-8.66. (ED 131-152)

The choice of educational outcome variables was also limited in data availability and was further constrained by other technical considerations (see Appendix G, Section I). Three such variables were selected--highest grade completed, postsecondary education (PSE) entry, and educational expectations. In addition to the importance of these variables within the goal structure of the UB program (both legislative and regulatory), such variables were available for the entire group of study participants (rather than for only a subset) and were those which had been shown to vary as a function of extent of UB participation (Chapter 4).

B. Basic Analysis Methodology

The basic approach to analysis was multiple regression, a very general statistical technique whereby the simultaneous relationship of several predictor variables to one outcome variable may be assessed. Generally, the technique allows the determination of that additive composite of the predictor variables which best predicts (shows the greatest zero-order correlation with) the single outcome measure.[5] In conducting the relational analyses, the individual sample member was the unit of analysis. For each of the outcome measures, several prediction models were examined. Such models were simply different mathematical formulations of how the composite of the predictor terms (used in predicting the educational outcomes) would be expressed.[6]

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[4] The base-year study used the project as the unit of analysis and aggregate student measures as outcome variables. The follow-up study provided greater statistical power for analysis by using the individual as the unit of analysis.

[5] The statistical technique of multiple regression is best suited for continuous variables; however, most of the predictor variables in this study are categorical in nature. The most common approach used in solving this problem is that of using dummy variables to reparameterize the prediction model. This involves the inclusion of k-1 dummy variables (coded as 0 or 1 and indicating specific category membership) for a categorical variable with k levels. Another less common technique is that of criterion scaling (see Appendix G, Subsection IV.A). Both of these approaches were used in these analyses.

For each criterion measure, the "full model" contained terms representing each of the predictor variables (specified above) and the pairwise products of those terms, which represent the first-order interactions among the variables.[7] Significant unique prediction by first-order interaction terms would suggest that prediction by some variables was differential depending on the particular value of the other variables. Of particular interest was differential prediction by extent of UB participation as a function of project association (suggesting differential project effectiveness). Other "reduced models," containing no interaction terms, were also considered. Through regression analysis, the various prediction models were compared toward obtaining the most efficient (parsimoneous) prediction system.

The relational analyses used sampling weights and procedures appropriate for weighted regression, within the multistage probability sample design of this study, whenever feasible. The regression procedures employed required complete data on all variables in the model, and a weighting class adjustment technique (see Appendix E, Section V) was used as an approach to problems of data indeterminacy.

C. Basic Analysis Strategy

The basic strategy of these analyses was to produce the most parsimonious prediction models explaining variation in the criterion measures;

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[6] A very simple model for predicting some variable, Y, from another variable, X, can be formulated as $\tilde{Y} = X$. More complex models can be created by addition of a constant term, other predictor variables, differential combination weights, and/or interaction terms. This would lead to a more complex right hand member of the equation, but the general form of the model would be the same (see Appendix G, Section III, for additional detail).

[7] A first-order interaction term is a term representing the joint contribution of two of the predictor variables beyond that contributed by either separately. For example, prediction may be increased by considering sex for each separate ethnicity classification, in addition to any separate unique prediction by sex and race. This would indicate a sex by ethnicity interaction (i.e., a differential prediction by sex among the different ethnicity classifications). Since major interest was in the possible existence of the first-order interactions (and due to certain technical considerations), higher-order interaction terms were not considered.

the approach used was to formulate a full model (i.e., a prediction model containing all predictor variables and all interaction terms) and then to reduce that model sequentially according to a predetermined set of steps and rules. Such an approach implies that the analysis questions posed above were to be answered in the reverse order of their presentation. At each step of the reduction, the reduced model was tested against the previous model, first by statistical test and then, if statistically significant, by magnitude test, to determine if the reduction in prediction (as a result of removing a particular set of terms from the model) was significant.[8] This sequential process ended when no additional terms (or set of terms) could be removed from the model without significantly reducing prediction. Once the simplified models were determined, they were examined to determine the manner in which the remaining predictor variables were uniquely related to the outcome measures.

D. Constraints and Limitations

The particular approaches used may be criticized on technical grounds in several respects (see Appendix G, Section I, for detail). The basic reason for choosing multiple regression as the analysis technique lies in its simplicity and the ease with which results can be interpreted. In general, the choice of all the techniques used in these analyses was based on these criteria (e.g., simplicity and ease of interpretability) and on a criterion of conservatism of the technique. Meeting these criteria was weighted more heavily than (and often precluded) strictly satisfying certain technical criteria. In practice, the techniques used are sufficiently robust and the sample sufficiently large to preclude stochastically any gross distortions of the results.

Beyond the strictly technical considerations, there are other limitations of analysis. These limitations are similar to those specified in Chapter 4, and they will be considered only briefly here. As detailed elsewhere in this report, the variables used in the regression analysis were not completely reliable, and the outcome measures are particularly time-bound. Further, the peculiar attenuation of the relationship between length of UB participation and high school completion is also reflected in the measures of educational progress--highest grade completed and postsecondary entry--used in these analyses, and, to the extent that educational expectations are partially tempered by reality, the attenuation may also extend to that criterion variable.

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[8] All statistical tests used the .05 level of significance as a criterion. The criterion for significance in the magnitude test was a 10 percent reduction in the proportion of variance explained. For example, if the proportion of variance explained by some model was .50, any reducted model explaining less than .45 (i.e., .50-(.1)(.50)) of the variance would be considered to have led to a significant reduction.

One final caution to the reader involves the inability of any of the relational analyses to establish causation. The fact that an outcome measure can be predicted by another variable (or by several other variables) does not mean that the predictor variable(s) can be considered as causing the outcome, even though the predictor variable(s) may logically precede the outcome variable in time. There is always the possibility that some additional unconsidered (or unmeasured) variable, which is related to the other variables under consideration, is the causal agent.

II. STABILITY OF RELATIONSHIPS WITHIN THE VARIOUS STUDENT·CLASSIFICATION CATEGORIES

This section presents the results of analyses conducted in addressing the third analysis question. That question is a very important one, both conceptually and as related to building predictive models. Effectively, the question posed is whether a predictive system can be built for any outcome measure that relies only on the individual values of the predictor variables or whether combined values of the variables must be considered. In other words, is the nature of the unique relationship between a particular predictor variable and an outcome measure more-or-less the same regardless of the value of other predictor variables (homogeneity of regression) or is the nature of that relationship moderated by the particular value of some other predictor variable (interaction of variables in prediction)?

One may reasonably expect interactions of the variables in predicting educational outcomes. It is certainly not unreasonable to expect that extent of UB participation would lead to different predicted outcomes depending on the project considered. Similarly, one might expect a given amount of UB participation to lead to different predicted outcomes depending on whether the student was male or female, poverty level or not, academic risk or not. Such interactions (differential prediction patterns) would suggest greater effectiveness of a particular amount of exposure to the UB program (including no exposure) at some project sites than at others or among certain specific student groups than among others. To the extent that such differential prediction patterns exist, simple models (involving only the predictor variables) would not be appropriate and more complex models (involving interaction terms) that allowed for such differential prediction would be necessary for a valid picture of the relationships.

Two separate but related approaches were used in investigating the possibility of differential prediction patterns. The first approach involved statistical tests for any unique prediction by first-order interaction terms of each of the separate outcome measures. These analyses involved a comparison of the full model (including all predictor variables and all possible first-order interactions involving those variables) against a model containing only the individual predictor variables (without interactions). This represents a simultaneous test of all interaction terms to determine if they added significantly greater predictability than could be obtained without them.[9] The second approach was less precise but also more straightforward. Separate regression equations were computed within each separate categorization of the sample members.[10]

In testing the full model (containing interaction terms) against a reduced model (not containing interaction terms), ordinary least squares solutions were used to obtain the regression parameters (in other words, sampling weights were not applied). Since the differential probability of individual selection is not reflected in the results, generalization beyond this specific set of individuals is inappropriate; however, for this particular step in model reduction generalizability was not a major goal. Instead, interest lay in determining existence of first-order interactions (and thus the existence of heterogeneity of regression). If such interactions existed, it is quite unlikely that they would not have been detected by the ordinary least squares solution.

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[9] This is analogous to a test of whether regression was or was not homogeneous within the various categories of students.

[10] Within a particular sample member category, the regression equation used all of the predictor variables except those related to the category under consideration (e.g., when considering the four categories of UB participation--0, 1, 2, or 3 years--all predictor variables except those involving UB participation were included in the predictor model).

The full model was less effective in predicting the criterion variables than one might expect, given the large number of terms (73) included. The percentages of total variability of the three outcome variables accounted for by the three full prediction models were:

a) highest grade completed--31 percent,
b) postsecondary entry--21 percent,
c) educational expectations--24 percent.

The most predictable outcome measure was highest grade completed, for which the prediction system of 73 terms accounted for less than one-third of the variation among the individuals in the sample. Stated somewhat differently, over two-thirds of the individual variation of any outcome measure could be attributable to factors other than those considered in the prediction system.

The test for stability of regression, the first step in attempted model reduction, involved the simultaneous removal of all interaction terms (61) from the prediction models, leaving three reduced models (one for each outcome measure) consisting of only 12 terms. The reduced models may be termed simple or "main effect" models since they involve only the basic predictor variables and no interaction terms. The percentages of total variability of the three outcome variables accounted for by the main effect models were:

a) highest grade completed--28 percent,
b) postsecondary entry--17 percent,
b) educational expectations--21 percent.

The reduction in predictability, as a result of removing all 61 interaction terms was no more than 4 percentage points. This is a surprising finding, which suggests that the increase in predictability of any of the three criterion measures by the addition of the 61 interaction terms is of little practical significance, and that the main effect models, involving only 12 terms, are almost as predictive as the full models. The finding is even more surprising in view of the fact that differential project effectiveness was reflected in the interaction terms involving extent of UB participation and specific project association.

The statistical tests performed to evaluate the increase in prediction attributable to the interaction terms indicated that the increase did not approach statistical significance for any of the criterion measures considered (see Tables G.6 through G.8 for summaries of these statistical analyses).

Since this finding was somewhat counterintuitive, additional analyses were conducted using the second approach outlined above. For each outcome measure, separate regression equations involving the basic predictor variables were computed within each of the student classifications. This involved computing 16 separate regression equations for each criterion measure, one for each student category considered.[11] For consistency with the previous analyses, these regression equations were also calculated using unweighted data. The results of the previously reported analyses imply that these separate equations should produce similar solutions.

The parameter estimates for these regression equations with associated standard errors are presented in Tables G.9 through G.11 for the outcome measures highest grade completed, postsecondary entry, and educational expectations, respectively. Visual examination of the regression weights for a specific prediction term over the 16 equations typically demonstrates the stability of the estimate, particularly when considering the magnitude of the standard errors associated with the estimates. Precise statistical tests for the overall differences in parameter estimates were not conducted; however, a relatively simple, but conservative, test of differences in estimates was made. That test involved the construction of 95 percent confidence intervals for each of the estimates[12] and the subsequent

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[11] The equations were computed within each of the 4 categories of extent of UB participation (0, 1, 2, or 3 years), each of the 3 categories of ethnicity (Black, white, and other), each of the 3 grade cohorts (10, 11, and 12), each category of sex (male, female), each category of poverty status (poverty level or not), and each category of academic risk (risk or not risk). Specific project association and age were treated as continuous, rather than categorical, variables and were not used to define student categories. The prediction term(s) associated with the particular categorical variable under consideration could obviously not be used in the equation.

[12] By invoking the Central Limit Theorem, the 95 percent confidence interval is represented by the value of the estimate ± two standard errors.

These tests were conducted, and the results are presented in Table G.12. There were some cases in which the confidence intervals did not overlap; however, some such instances would be expected by chance even if there were no true differences.[13] For any outcome measure considered, at least half (6) of the twelve prediction terms generated no instance of nonoverlap of the confidence intervals of the associated regression coefficients. The largest percentage of nonoverlapping confidence intervals (12.8 percent) was observed for the prediction term associated with the twelfth grade cohort in predicting educational expectations. The only prediction term consistently generating greater than 5 percent nonoverlapping confidence intervals for all three outcome measures was the term associated with one year of UB participation. This might have been expected, given the peculiarities associated with one year of program participation (see Chapter 4, Subsection II.B).

Although these analyses suggest that regression parameters associated with some specific prediction term may be less stable than desirable when considering a particular outcome measure, there is no indication of any generalized regression disparity among subgroups.[14] These results, therefore, support the previously reported results in indicating that regressions involving the main predictor variables were relatively homogeneous within different subgroups of individuals. As a result of the statistical tests, the full predictive model, including the interaction terms, was rejected for each of the outcome measures in favor of the simpler main-effect model that contained only 12 terms. The simpler model was only slightly less predictive than the full model.

While this finding allowed the use of considerably more straightforward prediction models, it remains counterintuitive. The finding suggests, for the limited number of intermediate education-related outcomes considered here, that the unique prediction of the outcome measures by extent of UB participation was more-or-less the same regardless of the particular project site involved or of the particular characteristics of the student. Stated less precisely, any estimated impact of extent of UB participation is effectively the same for Blacks, whites, or other ethnic groups, for males or females, for poverty level individuals or for those more fortunate, for academic risks as well as nonrisks, for tenth, eleventh, or twelfth graders, and--perhaps most counterintuitively--for whatever project site that was considered. Although the data are limited in many respects and although the analysis approach is not problem-free, this is a very unusual and powerful finding.

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[13] If all of the tests were independent, about 5 percent of nonconservative tests should be significant by chance even with no true differences. For conservative tests such as the ones performed here, the expected percent would be smaller. Since the tests were not independent, exact probability estimates cannot be made; however, the 5 percent figure is a reasonable rule of thumb.

[14] Recall that the test of interactions considered simultaneously all possible regression disparities for a particular outcome measure.

The finding of no interaction between extent of UB participation and specific project association warrants additional discussion. Since both participants and nonparticipants were represented in these analyses through the extent of program participation variable, the lack of significant unique prediction of outcomes by the interaction of project association with extent of UB participation suggests that projects do not differ from one another in effectiveness (as measured by the intermediate outcome variables considered), once differences in student characteristics, differences in length of program participation, and the contextual factors of specific project association have been taken into consideration.

This result supports the results of the base-year study, which produced basically the same finding. The follow-up study results are, however, even more compelling due to the greater statistical power obtained through the use of the student as the unit of analysis and to the additional consideration of the number of years during which an individual had participated in the UB program. Effectively this finding indicates that, although participation in the UB program is strongly associated with more desirable educational outcomes and though there is a zero-order relationship of specific project site with educational outcomes (see Tables G.3.1 through G.5.2), any differential effectiveness of specific projects can be explained by differences in the characteristics of students served by the project and/or by the different general context within which they operate.

III. UNIQUE PREDICTION BY SPECIFIC PROJECT ASSOCIATION

The analyses performed in addressing the second analysis question are reported in this section. That question involves the unique prediction of outcome measures by specific project association when considered jointly with the other predictor variables. There were both technical and conceptual reasons for addressing the prediction of specific project association as a separate issue. First, within the general scheme of model reduction, removal of specific project association would facilitate further analyses; specifically it would lead to a model in which a more appropriate variance estimation procedure could be employed.[15] Second, for nonparticipants and participants alike, this variable represents an important contextual factor (i.e., different geographical areas as represented by specific clusters of schools).[16]

Since the number of model parameters had been reduced to a workable size, a weighted least squares solution was used in evaluating the model. While this technique provides unbiased estimates of the parameters of the regression model, it tends to underestimate the standard errors of those parameters (and thus to overestimate the strength of the contribution to prediction by a particular predictor term). The percentages of variance of the three outcome measures accounted for by the main-effect models, as solved by weighted least squares, were:

a) highest grade completed--23 percent,
b) postsecondary entry--15 percent,
c) educational expectations--17 percent.

As can be seen by comparing these results to those presented in the previous section, the unweighted ordinary least squares solution had overestimated the strength of all three relationships.

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[15] That approach is a Taylorized deviation approximation, which uses within-strata estimates of error. Project membership can take on only two possible values in each stratum, since only two projects were selected per stratum.

[16] For UB participants it also represents the different UB projects of the study (together with attendant differences in project approaches to intervention); however, differential project effectiveness was excluded from the predictive model with the exclusion of the interaction between extent of UB participation and specific project association. Thus, the remaining project association factor is a contextual variable exclusively for both participants and nonparticipants.

After removing the contextual variable of specific project association from the prediction system, the percentages of variance of the three outcome variables accounted for by the reduced main-effect models were:

a) highest grade completed--23 percent,
b) postsecondary entry--14 percent,
c) educational expectations--16 percent.

It can be seen that removal of specific project association from the models had little effect in reducing the proportion of explained variance in any absolute or relative sense. Moreover, the statistical test for this reduction did not approach significance (see Tables G.13 through G.15 for tabular summaries of the statistical tests). The contextual variable represented by specific project association was therefore removed from the model as not contributing any unique prediction (beyond that contributed by other factors in the model) to the specific educational outcome variables under consideration.

This finding suggests that any variation of the outcome measures among sites is equally explainable by differences in students among those sites. This is similar to a fairly common finding in educational research: namely, that between site variation is considerably less than variation among students within site.

IV. FURTHER REDUCTION OF THE PREDICTION MODELS

With the removal of project membership from the prediction equations, relatively straightforward main effects models remained. The three prediction systems remaining were ones in which variations in the educational outcomes under consideration were predicted solely by student characteristics and by temporal factors--their age and their starting point in the educational system in academic year 1973-74. Further, removal of specific project membership allowed the use of more appropriate procedures in estimating the strength of the relationships and the relative contributions of the terms remaining in the reduced main-effect models. The models were further reduced, when possible, by removing the full set of generated contrasts (predictor terms) for each predictor variable. The order in which terms (or sets of terms) were removed from the three models was determined by their respective relative increase in explained variation of the outcome variable.

It was possible to reduce all of the models by removing at least one but no more than two of the remaining predictor variables in the predictor variable set. The only additional factors removed in any model were poverty level and sex. (See Tables G.16.1 through G.18 for summaries of additional model reduction steps.) In terms of the weighted least squares solutions, the percentages of variance of the three criterion measures accounted for by the final reduced models were:

a) highest grade completed--22 percent,
b) postsecondary entry--13 percent,
c) educational expectations--16 percent.

These reduced models are discussed in detail in the following section.

V. THE FINAL REDUCED MODELS

The parsimonious models resulting from the various steps of model reduction were evaluated for the nature of their prediction. The predictor variables remaining in the models for all three outcome measures were extent of UB program participation, race, grade level in 1973-74, age, and academic risk status. In addition, the model predicting educational expectations contained poverty status as a predictor variable, and the model predicting postsecondary entry contained sex as a predictor variable. The specific regression parameters (combination weights) for the three prediction equations are given in Table 5.1.

The specific coding of the predictor terms in these models is important for an understanding of the prediction weights. The categorical variables were so coded that the constant term in the regression equation (the intercept) represents the average value of the outcome variable for a specific base group; namely, that subset of individuals classified as white, male nonparticipants who were 16 years old and in the eleventh grade during academic year 1973-74 and who were not poverty status and not risk status (see Table G.1 for details). The regression weights assigned to other categories (e.g., females, those with 1 year participation, academic risks, etc.) as represented by various predictor terms give the unique contribution of such other classifications

Table 5.1

REGRESSION EQUATIONS FOR PREDICTING THREE EDUCATION-RELATED OUTCOMES

Outcome Predicted	Intercept[a]	Regression Weights for Predictor Terms[b]										
		Extent of UB Participation			Ethnicity		Grade Cohort					
		1 Year	2 Years	3 Years	Black	Other[c]	10th	12th	Age[d]	Academic Risk Status[e]	Sex[f]	Poverty Status[g]
Highest grade completed	12.70	+.07*	+.35	+.39	+.14*	-.17*	-.38	+.82	-.27	-.42	[h]	[h]
Postsecondary entry[i]	0.63	+.05*	+.18	+.26	+.01*	-.15	-.08*	+.14	-.09	-.21	+.11	[h]
Educational expectations[j]	3.37	+.33	+.63	+.81	+.48	-.22*	-.36	+.51	-.37	-.56	[h]	-.38

NOTE: The regression equations presented in this table were solved using weighted data and the weighted least squares criterion for those cases in which no data indeterminacy existed for either the full predictor variable set or for the specific outcome measure being considered.

[a] The predictor variables were coded in such a way that this parameter represents the predicted value of the specified outcome for a base group, specifically, white, nonrisk, male nonparticipants who were not within poverty level and who were 16 years old and in the eleventh grade during the 1973-74 school year.

[b] With the exception of age, the predictor terms are dummy variables reflecting group membership as indicated in the column label or in a footnote. These terms have been grouped to reflect their relationship to the categorical variables from which they were derived. The weights given may be applied directly to the value of the term under consideration. Specific predictor terms that do not show a statistically significant ($p<.05$) departure from 0 are indicated by an asterisk (*).

[c] Other than Black or white.

[d] Age reflects age in the spring of 1974 and has been recoded by subtraction of 16; the recoded variable thus represents years over or under expected eleventh-grade age.

[e] The category "academic risk" is the specific prediction term.

[f] The category "female" is the specific prediction term.

[g] The category "within poverty status" is the specific prediction term.

[h] The specified categorical variable did not contribute significantly ($p<.05$) to the prediction of the outcome measure of concern.

[i] The specific coding of this variable allows its predicted value to be interpreted as a likelihood of postsecondary entry.

[j] The scale of this variable is such that a value of 1 indicates an expectation of high school completion only; a value of 4 or 5 indicates four-year college completion and completion of graduate training, respectively; and a value of 2 or 3 indicates expectation intermediate to these extremes.

(either incremental or decremental) in predicting the criterion measure relative to the base group.

It is also important to recognize that while all of the variables (e.g., race, sex, UB participation) remaining in the final prediction models made statistically significant ($p<.05$) contributions to prediction, some of the regression weights assigned to specific predictor terms were not significant when considered singly.[17] An asterisk (*) is used in Table 5.1 to designate any weight that did not, in itself, reach statistical significance.

The similarity of the regression models for the three outcome measures is quite evident from Table 5.1. The incremental or decremental nature of the regression weights for a given prediction term (reflected in the sign of the weight) are the same, regardless of the outcome being considered. Although a direct comparison of the absolute value of the regression weights across models is not appropriate due to differential variability in the respective outcome measures, it can be seen from Table 5.1 that the relative magnitude of a particular weight (as compared to other weights in the same prediction equation) is similar from model to model.

The temporal variables, age and grade cohort, are related to the outcome measures in more-or-less expected fashions. The predicted values of the outcome measures are reduced for members of the tenth-grade cohort and increased for members of the twelfth-grade cohort, all other things being equal. The magnitude of this increase or decrease is not as great as it first appears, when one considers that age generally changes as a function of grade cohort. When one also corrects for the typical age within grade, increments and decrements in prediction associated with grade cohort are offset to some extent by corresponding decrements and increments associated with age. Being over age-in-grade is associated with a reduction in predicted outcomes while being under age-in-grade leads to an increase. Being classified as an academic risk consistently surpresses the predicted value of the outcome measures, as does ethnic classification as neither white nor Black (although the decrease in prediction associated with this latter classification is statistically significant only in the case of postsecondary entry). Ethnic classification as Black, on the other hand, is associated with increased predicted values of the outcome measures, but this increase is significant only when considering educational expectations. Participation in the UB program is associated with higher predicted values of the educational outcomes, and greater participation in the program is associated with greater increases in prediction. One year of program participation yields a significant increase in prediction only when considering educational expectations. Sex and poverty status classification provide significant unique prediction in only one of the three prediction models.

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[17] It is not the test of an individual prediction term (a dummy variable) weight that determines the significance of a variable; rather variable significance is determined by a simultaneous test of the weights assigned to all the prediction terms generated for that variable. As an example, the variable, extent of UB participation, generated three dummy variables representing three separate prediction terms; evaluation of the contribution to prediction by extent of UB participation would involve a test of all three terms taken as a set.

A. Predicting Highest Grade Completed

The reduced regression model for predicting highest grade completed is described in Table 5.1. It can be seen from that table that being an academic risk or over age-in-grade is associated with reduced educational progress. Initial grade, which imposes an upper limit on current educational progress, seems to increase or decrease predicted highest grade completed in the appropriate direction even when considering differences in typical age within grade. The increment in predicted educational progress associated with the twelfth-grade cohort is, however, considerably greater than the decrement associated with the tenth-grade cohort. Ethnic classification is also significantly associated with predicted highest grade completed, even though neither of the ethnic-group-specific regression weights shown in Table 5.1 are significant departures from 0 when considered separately. The general prediction tendency (which should be considered relative to the white base group) is that classification as Black is associated with greater educational progress and that classification as neither Black nor white is associated with less educational progress. The fact that poverty status classification and sex do not appear in this prediction system indicates that they contributed no significant unique prediction beyond that accounted for by other variables in the model.

Participation in the UB program is associated with increased educational progress; and longer program exposure is associated with greater increments. While the contribution to prediction by one-year participation in the program is positive, it does not differ significantly from 0; however, the increase in prediction associated with two- or three-year program participation tends to offset the decrease in prediction due to being an academic risk or that of being over age-in-grade. Although the difference in the prediction increments from two-year UB participation to three-year participation were not evaluated for significance, the absolute and relative differences are small and 95 percent confidence intervals about these weights show considerable overlap (see Table G.19 for associated standard errors).

Using the prediction equation given in Table 5.1, values of highest grade completed were estimated for selected subgroups of individuals. These predicted values are given in Table 5.2 to demonstrate how changes in student classification are reflected in the predicted values (expectations) of educational progress.[18]

The first row of Table 5.2 gives the predicted highest grade completed for the base group--white eleventh graders, at age-in-grade (16), with no UB participation, who are not academic risks. In three years, this group can be expected, on the average, to have completed high school and almost a full year of PSE (predicted highest grade completed of 12.70). If, however, one considers the group of eleventh-grade nonparticipants who are one year over age-in-grade, academic risks, and of an ethnicity other than white or Black (row 6), the expected educational progress is less than high school completion (predicted highest grade completed of 11.84). Allowing two years of participation in the UB program for this latter group (row 7) increases the expected progress to include high

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[18] In examining these predictions it should be kept in mind that the predicted value for a particular classification of individuals carries an associated standard error of prediction, which is not reported here (see, however, Table G.19) but which reflects both sampling error and the fact that prediction is for a group rather than for an individual in that group. These data are presented simply as a demonstration of the ways in which different classifications affect prediction, and do not include all possible classifications. Additional predictions can be obtained by the interested reader for any group of interest by employing the equation given in Table 5.1.

Table 5.2

PREDICTED VALUE OF HIGHEST GRADE COMPLETED FOR SELECTED CATEGORIES OF INDIVIDUALS

Classification of Individual					Predicted Highest Grade Completed
Grade	Age	UB Participation	Ethnicity	Academic Risk Status	
11	16[a]	None	White	Not Risk	12.70
12	17[a]	None	White	Not Risk	13.25
10	15[a]	None	White	Not Risk	12.59
11	16[a]	None	White	Risk	12.28
11	16[a]	None	Other than Black or White	Risk	12.11
11	17[b]	None	Other than Black or White	Risk	11.84
11	17[b]	2 Years	Other than Black or White	Risk	12.19
11	17[b]	3 Years	Other than Black or White	Risk	12.23
11	16[a]	None	Black	Not Risk	12.84
11	16[a]	None	Black	Risk	12.42
11	16[a]	3 Years	Black	Risk	12.81
11	15[c]	3 Years	Black	Not Risk	13.50

NOTE: Predicted values were computed using the regression weights given in Table 5.1.

[a] At typical age for grade level.

[b] One year over typical age for grade level.

[c] One year under typical age for grade level.

school completion and PSE entry (predicted highest grade completed of 12.19).

The first three rows indicate the variation in highest grade completed that is attributable to the initial educational starting point (correcting for typical age-in-grade). The final row in Table 5.2 indicates that Black eleventh graders who were one year under age-in-grade, who were not academic risks, and who participated in the UB program for three years would be expected to show approximately normal uninterrupted educational progress during the three year period (e.g., to have completed the first year and part of the second year of PSE--predicted highest grade completed of 13.50).

B. Predicting Postsecondary Entry

Postsecondary entry was scaled as a (0,1) variable with 1 representing entry; as such, the predicted value of PSE entry can be roughly interpreted as the likelihood of entering PSE. The solution of the reduced main-effect model for predicting likelihood of postsecondary entry is given in Table 5.1 (for additional detail, including standard errors, see Table G.20). This prediction equation is similar in many ways to the previously presented equation; however, in predicting PSE entry, there is a marginally significant increment associated with being female. Also, the increments and decriments associated with different grade cohorts are offset considerably more when one considers the typical age-in-grade. Further, in this equation the decrease in prediction as a result of being of some ethnic classification other than Black or white represents a significant decrement. Poverty status classification does not appear in the model, since it did not contribute significantly in predicting postsecondary entry, after accounting for the prediction of other terms in the model.

The slight increase in predicted likelihood of PSE entry due to one year of UB participation carries no statistical significance. Participation in the program for either two or three years does add significantly to predicted likelihood of PSE entry; three years of participation in the UB program is associated with an increase in the predicted probability of PSE entry of 25 percentage points. Although exact tests for differences between the regression weights for two years' and three years' UB participation were not conducted, the 95 percent confidence intervals about the two weights show some overlap. The increase of predicted postsecondary entry as a result of two or three years of UB program participation offsets the decrease in prediction associated with academic risk or of being over age-in-grade.

The expected likelihoods of PSE entry for selected categories of individuals are presented in Table 5.3. The first three rows of the table suggest that there is little variation in predicted likelihood of PSE entry (within three years) among the various grade cohorts, when correcting for typical age-in-grade. The likelihood of PSE entry for the base group (white, male nonparticipants, who were not academic risks and who are at typical age, 16, within the eleventh grade) is .63. For the group of eleventh grade, male nonparticipants who are academic risks, one year over age-in-grade, and neither Black nor white, the likelihood of PSE entry is considerably reduced to .18. For this latter group, allowing three years participation in the UB program increases the predicted likelihood of PSE entry to almost the level of a 50/50 chance (.44).

The danger of over interpretation of the prediction models is clearly demonstrated in the last row of Table 5.3, which indicates a predicted likelihood of PSE entry of 1.09 for eleventh-grade, white, nonrisk females who are one year under age-in-grade (15) and who participate in the UB program for three years. Obviously, the probability of PSE entry cannot exceed 1; however, the predicted value for a given group is not constrained by that limiting value. Rather, predicted values for any group value are those that when taken simultaneously with the predicted values of all other groups minimizes a function of the total errors of prediction.[19] The predicted value for this group carries an associated standard error, and the value of 1 would probably be included within the 95 percent confidence interval. In any event, the predicted value indicates that the likelihood of PSE entry within this group is extremely high, approaching certainty.

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[19] More precisely, the solution of the regression equation minimizes the weighted sum of squared deviations from predicted values (the weighted least squares criterion).

Table 5.3

PREDICTED PROBABILITY OF POSTSECONDARY ENTRY FOR SELECTED CATEGORIES OF INDIVIDUALS

Classification of Individual						Predicted Probability of Postsecondary Entry
Grade	Age	UB Participation	Sex	Ethnicity	Academic Risk Status	
11	16[a]	None	Male	White	Not Risk	0.63
12	17[a]	None	Male	White	Not Risk	0.68
10	15[a]	None	Male	White	Not Risk	0.64
11	16[a]	None	Male	White	Risk	0.42
11	17[b]	None	Male	White	Risk	0.33
11	17[b]	None	Male	Other than Black or White	Risk	0.18
11	17[b]	1 Year	Male	Other than Black or White	Risk	0.23
11	17[b]	2 Years	Male	Other than Black or White	Risk	0.36
11	17[b]	3 Years	Male	Other than Black or White	Risk	0.44
11	16[a]	None	Female	White	Risk	0.53
11	16[a]	3 Years	Female	White	Risk	0.79
11	15[c]	3 Years	Female	White	Not Risk	1.09[d]

NOTE: Predicted values were computed using the regression weights given in Table 5.1. The specific coding of this outcome measure allows its predicted value to be interpreted as a likelihood of postsecondary entry.

[a] At typical age for grade level.

[b] One year over typical age for grade level.

[c] One year under typical age for grade level.

[d] Obviously, a probability cannot be greater than 1; however, the predicted value is one that, when taken simultaneously with all other possible predicted values, minimizes the total errors of prediction.

C. Predicting Educational Expectations

Interpretation of the reduced model for predicting educational expectations of individuals is clouded somewhat by certain scale inadequacies of the educational expectations variable (see Appendix D, Section VI). The value of 1 on that scale indicates an expectation of high school completion only; the values 4 and 5 indicate four-year college completion and completion of graduate training, respectively; and the scale values 2 and 3 indicate educational expectations intermediate to the extremes. The summary of the solution to the regression model for this outcome is given in Table 5.1 (additional details, including standard errors, are given in Table G.21).

It can be seen that the directionality of the weights are the same as for other models presented. The weights for the different grade cohorts appear, at first glance, to suggest different educational expectations as a function of grade, which does not agree with the results reported in Chapter 4. When one corrects for the age differential across grade, however, compensations for the cohort differences are realized (e.g., predicted educational expectations for sixteen-year-old eleventh graders are the same as those for fifteen-year-old tenth graders). The major temporal contribution to the prediction of educational expectations is thus seen to be associated with being out-of-age within grade (over age individuals having lower expectations and under age individuals having higher expectations). The predicted increase in expectations for Blacks over whites is a significant one (all other things being equal), but the decrement in prediction for those of other ethnic groups is not significant. As was the case for other models, predicted educational expectation is reduced for those who are academic risks. The model for educational expectations differs from previously reported prediction models in that poverty status provides statistically significant unique prediction. All other things being equal, educational expectations are less for individuals classified as poverty level. Sex added no unique prediction of educational expectations.

Extent of UB program participation is strongly related to predicted educational expectations. Even a single year of participation in the UB program is associated with a statistically significant increase in prediction (for the two previous prediction models considerd, one year of UB participation was not associated with a significant increase). Further, the increase in prediction is more clearly related to the length of program participation than was the case in previous prediction models that have been considered. The increase in prediction associated with three years of program participation is almost large enough to compensate for the combined decrease in prediction associated with being an academic risk and within poverty level.

As a demonstration of the extent to which predicted educational expectations vary as a function of group classification, predicted values were computed for selected classifications of individuals. These results are presented in Table 5.4. The first three rows of that table demonstrate the minimal effect of grade cohort in predicting educational expectations, when correcting for age-in-grade. The predicted expectations for individuals in the base group (white, nonrisk nonparticipants who are at age-in-grade, 16, within the eleventh-grade cohort and who are not poverty level) approaches four year college completion (predicted value of 3.37). The predicted value of educational expectations for those eleventh-grade nonparticipants who are one year over age-in-grade, academic risks, within poverty level, and classified as neither Black nor white is considerably lower (1.84), indicating that a large porportion of this group expect only high school completion (or less). When those in this latter group are exposed to three years of UB participation, predicted educational expectations are raised to include, on the average, some postsecondary work (predicted value of 2.65). The last row of Table 5.4 serves as another example of the predicted value for a particular group exceeding the maximum value of the variable under consideration.

VI. SUMMARY

This chapter has presented an examination of three supplementary analysis questions concerning the relationships to certain measures of educational progress, persistence, and expectations of individual characteristics, temporal factors, contextual factors, and extent of UB program participation. There are several recognized constraints on the interpretation of results presented in this chapter, due in part to weaknesses

Table 5.4

PREDICTED VALUE OF EDUCATIONAL EXPECTATIONS FOR SELECTED CATEGORIES OF INDIVIDUALS

Classification of Individual						
Grade	Age	UB Participation	Ethnicity	Poverty Status	Academic Risk Status	Predicted Educational Expectations
11	16[a]	None	White	Not Poverty	Not Risk	3.37
12	17[a]	None	White	Not Poverty	Not Risk	3.51
10	15[a]	None	White	Not Poverty	Not Risk	3.38
11	16[a]	None	White	Poverty	Not Risk	2.99
11	16[a]	None	White	Poverty	Risk	2.43
11	17[b]	None	White	Poverty	Risk	2.06
11	17[b]	None	Other than Black or White	Poverty	Risk	1.84
11	17[b]	1 year	Other than Black or White	Poverty	Risk	2.17
11	17[b]	2 years	Other than Black or White	Poverty	Risk	2.47
11	17[b]	3 years	Other than Black or White	Poverty	Risk	2.65
11	16[a]	None	Black	Poverty	Risk	2.91
11	16[a]	3 years	Black	Poverty	Risk	3.72
11	15[c]	3 years	Black	Not Poverty	Not Risk	5.03[d]

NOTE: Predicted values were computed using the regression weights given in Table 5.1. The scale of this variable is such that a value of 1 indicates an expectation of high school completion only; a value of 4 or 5 indicates four-year college completion and completion of graduate training, respectively; and a value of 2 or 3 indicates expectation intermediate to these extremes.

[a] At typical age for grade level.

[b] One year over typical age for grade level.

[c] One year under typical age for grade level.

[d] This predicted value is greater than the maximum scale value; however, the predicted value is one that, when taken simultaneously with all other possible predicted values, minimizes the total errors of prediction.

in the measures and in part to design and methodological limitations. Nonetheless, the major findings are internally consistent and consistent with both the base-year study results and the results reported in Chapter 4. The support from prior results and the basic robustness of the analysis techniques used lend credibility to the findings, even though they should be considered as tentative.

The purpose of pursuing these additional relational analyses was to allow a perspective from which to view the UB program that was different from that afforded by the comparative descriptive results reported in Chapter 4. Rather than addressing questions of the form, "Do educational outcomes vary as a function of UB participation?", this chapter has addressed more inclusive questions such as: "What variation in educational outcomes is explainable by poverty status and/or academic risk status?", "Is such variation greater or less than that accounted for by length of UB participation?", "Is specific project association related to predictable variation in educational outcomes?", and "Are the relationships between extent of UB participation and educational outcomes modified in any way as a function of specific UB project association (suggesting differential project effectiveness)?" The regression approach to these analyses provides a more holistic and integrative depiction of the UB process than was possible in the approach employed in Chapter 4. The two approaches are nevertheless complementary, and, taken together, they provide a more complete picture of the UB program than that provided by either methodology alone.

Three outcome measures were considered in these analyses: highest grade completed, postsecondary entry, and educational expectations. The variables used to explain variation in those outcomes (predictor variables) were extent of UB participation, specific UB project association, sex, race, poverty status, academic risk status, as well as age and grade level during the 1973-74 academic year. The major findings can be classified: (a) implications deriving from the fact that certain predictor variables did not demonstrate meaningful unique explanatory power in accounting for variation in outcome measures, and (b) implications deriving from the nature of the relationships between outcome measures and those variables that did demonstrate unique explanatory power. These different classes of findings are discussed separately below.

The most surprising finding was that the relationships between the three outcome measures and the predictor variables were quite stable regardless of the particular project site involved or the particular characteristics of the sample member (i.e., none of the relationships were interactive). Although the relationships of predictor variables to outcomes were weaker than might have been expected (accounting for at most 31 percent of outcome variability--e.g., a multiple correlation of .56), the increase in explanatory power by allowing for interactive relationships was negligible in both an absolute and a statistical sense. In other words, once the complex of predictor variables (sex, race, age, risk status, poverty status, grade, extent of program participation, and specific project association) is considered, any additional level of explanation of outcome measures afforded by considering interactive relationships is effectively nonexistent.

A particular relevant implication from this finding is that any suggested impact on these intermediate educational outcomes attributable to UB participation is effectively the same for Blacks, whites, or other ethnic groups; for males or females; for poverty level and nonpoverty individuals; for academic risks as well as nonrisks; for tenth eleventh, or twelfth graders; and for whatever project site considered. The lack of an interactive relationship between project association and extent of participation in the UB program is of particular importance since it implies that differential project effects can be discounted in predicting the outcome measures selected. That is, when student characteristics, temporal factors, contextual factors, and length of program participation are accounted for, there are no detectable differences between projects in the selected outcome measures. This finding supports a similar finding reported in the base-year study, in which it was shown that projects do differ in types of operation, types of students, and relative success in certain outcome measures but that when one controls for student input characteristics, project outcome differences are negligible. For the rather general intermediate outputs selected for these analyses, any impact of UB program participation can be considered free of any differential impact associated with specific project processes. This finding considerably simplifies the prediction models and allows a much clearer picture in interpreting subsequent findings.

Beyond the reflection of differential project effectiveness (expressed in the interactive relationship to outcomes by the combined effect of extent of UB participation and specific project association), the project association variable also represents an important contextual factor (i.e., different geographical areas as respresented by specific clusters of schools). It was found that this contextual (or site) factor did not demonstrate any appreciable added explanatory power in accounting for variability of any of the three educational outcome measures. That is, any variation of educational progress, persistence or expectations as a function of site considered is equally explainable by differences in either individual characteristics, temporal factors, or length of program participation among those sites. The finding that variation between sites is small relative to variation of individuals within sites is a fairly common finding in educational research.

By discounting any added explanatory power of interactive relationships or of contextual factors, the remaining prediction system for each outcome measure consisted of the variable associated with the UB process (extent of program participation), and those variables concerning individual characteristics or temporal factors. These systems were further examined for possible redundancy (nonuniqueness) of the prediction attributable to the remaining variables. With the exception of sex and poverty status, all remaining predictor variables demonstrated significant unique explanatory powers in accounting for variation in all three of the outcome measures.

In general, the proportion of variability of educational outcomes accounted for by the predictor variables was not great (22 percent of the variation in highest grade completed, 13 percent of the variation in probability of postsecondary entry, and 16 percent of the variability of educational expectations). The relative weakness of the prediction systems indicates that additional unconsidered or unmeasured factors account for the large majority of individual variation in these educational outcomes. Even though the relationships were not as strong as would be desired, they did account for a significant amount of outcome variation (in both a statistical and practical sense). Moreover, the forms of the prediction equations were informative in gaining an understanding of the extent to which UB participation compensated for decrements in predicted outcomes associated with various forms of educational or cultural disequalization.

The solutions for the prediction equations were quite similar for each outcome considered. For each of the outcomes the regression weight associated with a specific predictor variable was consistently in the same direction and of a similar relative magnitude as analogous regression weights for other outcomes. For each outcome measure, increased exposure to the UB program was associated with improved predicted outcomes (i.e., greater educational progress, greater persistence through entry into PSE, and higher expectations). One anticipated detriment to progress/persistence and expectations, academic risk status, lead to a reduction in all predicted outcomes; however, another anticipated detriment, poverty status, was associated only with lower educational expectations. Ethnicity was related to all outcomes. Blacks experienced greater progress/persistence and higher expectations than whites; other ethnic categories experienced less progress and lower expectations than either whites or Blacks. Sex classification, on the other hand, was sigificantly related only to postsecondary entry; females were predicted to have a greater likelihood of PSE entry.

The two temporal variables, age and grade level in 1973-74, were also significantly related to all three outcomes, but with the exception of highest grade completed (for which there are obvious limits imposed by initial grade level), the effect of the age factor tended to nullify the effect of grade cohort. That is, although both grade level and age demonstrated unique predictive power, all other things being equal, when one considered the typical age for the grade being considered, initial grade level did not increment or decrement PSE entry or educational expectations in any appreciable or systematic manner. The important temporal variable was thus seen to be whether an individual was out-of-age within grade. Under age-in-grade was consistently associated with incrementing outcomes while over age within grade was associated with a decrease in predicted outcomes.

The most interesting findings from these models, however, deal with the relative magnitudes of the contributions to prediction by extent of UB participation within the three predictive systems. Although exposure to the UB program was associated with increments in educational progress/

persistence as well as expectations; the increment associated with one-year participation was only significant in predicting educational expectations. The difference in outcome increments attributable to two-year and three-year program participation was typically small, being most pronounced for educational expectations. These results are consistent with those reported in Chapter 4. Moreover, for all outcome measures, the increment in outcome attributable to three years' participation in the UB program compensated for the decrement in that outcome due to classification as an academic risk. When considering educational expectations, three years of UB participation compensated almost completely for the combined decrement of academic risk status and poverty-level classification. In no case, however, did maximum UB participation compensate for the combined decrements of all factors that served to reduced values of predicted outcomes.

In general, these findings support and complement both the base-year findings and the results reported in Chapter 4. It is clear from the relative magnitude and statistical significance of the regression weights that continued exposure to the UB program is associated with increases in estimated educatonal progress, persistence, and expectations. A single year of program participation is associated with a significant increase in expectations, but no substantial relationship was detected between such minimal program exposure and educational progress and persistence. Further, with the exception of expectations, the gain in outcomes associated with three years of program participation is only marginally greater than the gain associated with two years of participation.

The findings also demonstrate that any incremental effect associated with program exposure can be masked if comparisons are made to nonparticipants who differ in respect to other basic variables (individual characteristics and temporal factors), since the initial status of the student is significantly associated with educational outcomes. The regression analyses also suggest that exposure to the program can, in some instances, compensate for certain states (or combinations of states) of educational and social deprivation but that it cannot compensate for all such states.

Chapter 6

Summary and Conclusions

I. BACKGROUND, PURPOSE, AND METHODOLOGY

A. Background

Under authority of the Economic Opportunity Act of 1964, as amended (42 U.S.C. 2809), the Office of Economic Opportunity (OEO) funded 17 Upward Bound (UB) projects as a pilot program in the summer of 1965. In 1966, UB was authorized as a national program under Title II.A of the Economic Opportunity Act. On July 1, 1969, the UB program was transferred from OEO to the U.S. Office of Education (USOE), Department of Health, Education, and Welfare. The present legislative authority is the Education Amendments of 1976 (Public Law 94-482).

The program was designed to reach low-income high school students who have potential for successfully completing an educational program, but who, due to inadequate preparation and/or lack of motivation, are prevented from meeting conventional criteria for admission to a postsecondary program. Through the use of various intervention strategies (e.g., remedial instruction, exposure to new or altered curricula, tutoring, cultural enrichment activities, counseling, and encouragement), the program is legislatively mandated to generate in participants the skills and motivation necessary to enter and successfully complete postsecondary education (PSE).

B. Purpose

The research reported here is a first follow-up of a prior (base-year) study of the UB program. The base-year study provided a comprehensive description of the UB program as it existed in program year 1973-74, investigated the relationships of UB participation on short-term educational outcomes, and examined the relative effectiveness of different types of UB projects in respect to these short-term outcomes. The earlier study was constrained, however, to investigating educational outcomes only within the specific grade cohorts considered (tenth, eleventh, and twelfth graders in the fall of 1973) over a one-year period and provided no information regarding postsecondary progress and persistence.

One major purpose of the follow-up study was an examination of the association between educational progress and persistence (particularly in connection with the postsecondary experience) and extent of participation in the UB program. Another major purpose of the follow-up study was to examine the unique relationships to certain educational outcome measures of student characteristics (race, sex, poverty status, academic risk status), temporal factors (age and grade cohort), and contextual factors (specific project association) in addition to extent of UB participation, to gain a fuller understanding of potential program effectiveness in respect to these associated factors. Further, the follow-up study allowed the investigation of student expectations of continued education, the problems experienced in obtaining a desired level of education, and other outcomes not directly related to education.

C. Design

The basic design of the follow-up study was necessarily constrained by the design of the preceding base-year study; the samples of UB participants and nonparticipants had been drawn during the base-year study and many of the variables of major theoretical interest had been defined and measured at that time. The base-year study focused on the 333 regular UB projects operating in the coterminous United States during the 1973-74 program year. From these regular UB projects, 54 were selected after stratification on student ethnicity, number of students served, project location, type of project, and type of host institution. All program participants in the sampled projects who were in grades 10, 11, or 12 were selected, yielding 3,710 UB participants in the final sample. For each selected UB project, an average of two high schools providing students to that project were selected, and, from a sample of classrooms in each of these schools, a total of 2,340 nonparticipating comparison students (about 21 per sampled school) were selected after stratification on grade level, ethnicity, low-income status, and academic risk.

The follow-up study was designed to address questions regarding intermediate educational outcomes, which could not be considered in the base-year study due to temporal limitations. The study involved data collection during the winter and early spring of 1977 from members of the base-year study sample. The information collected was minimal and related to the status of sample members at that time.

D. Method

The follow-up survey consisted of two mailings of a self-administered postcard questionnaire to 5,973 eligible base-year sample members and of telephone interviews with a probability sample of 2,500 nonrespondents to these mailings. The data collection instrument used in the follow-up study was designed to collect only the information pertinent to specific data needs of USOE with a minimum burden on the respondent.

In addition to data obtained from the survey instrument, other data were obtained from existing sources and merged with those collected through the questionnaire. Student background information, prior school status, and prior UB participation status were available from the student master file developed during the base-year study. Information characterizing the postsecondary education institutions attended by sample members was obtained from an institutional data file created by the College Entrance Examination Board (CEEB) and from records maintained by the Division of Student Services and Veterans Programs of USOE.

Data collection, processing, and analysis activities took place from January through July 1977. Of the 5,973 individuals eligible for the survey, information was obtained for 4,221 for a total response rate of 71 percent. Sample coverage was considerably higher (95 percent) due to the very high contact rate among the mail nonrespondents subsampled for telephone interview. Data were generally more available for former UB participants than for nonparticipants, reflecting, in part, the greater ease with which they could be traced through their previous project director; however, differences in data availability among the three grade cohorts were relatively small and were not systematic.

Due to the several efforts to collect complete data during the follow-up study, the amount of missing data for items of the follow-up instrument was small. Data indeterminacy as a result of missing data in the CEEB institutional data file was, however, considerably greater (data availability varied from a low of 62 percent to a high of 87 percent).

Data inconsistency (either within the follow-up study instrument or between follow-up and base-year data) was minimal with the exception of extent of UB participation, for which an excessively large number of inconsistencies existed between the follow-up and base-year measures. Such inconsistencies were suspected to be primarily a function of the imprecision of the base-year measure. Data reliability was estimated to be within acceptable limits.

E. Data Analysis

Two conceptually different, but related, approaches to analysis were undertaken. The first approach may be characterized as comparative descriptive analysis, quite similar in form to the analyses performed in the base-year study. These analyses addressed questions of national program impact and involved the comparison of educational outcomes of UB participants with those of nonparticipants after statistically equating the two groups on relevant background variables. The second approach may be characterized as relational analysis. Using the individual as the unit of analysis, multiple regression was employed to predict outcomes on the basis of individual background factors, temporal and contextual factors, and extent of program participation.

In conducting analyses, sampling weights were used to account for unequal selection probabilities. Weight adjustment procedures were implemented so that nonrespondents would be represented in the final results by respondents who were most like them. Appropriate standard errors were computed for all comparative descriptive results and, when feasible, for the relational results.

F. Limits and Constraints Upon Results

Throughout the study, care was taken to avoid techniques of data collection, processing, or analysis that would introduce bias into the study results; equal care was taken in interpreting study findings. There are, however, certain limitations imposed on the findings that reflect, in the main, limitations of the data or of the procedures used in conducting the study; some additional constraints stem from the original base-year design and the limited changes that were possible in a follow-up study. Moreover, the data were collected through survey methodologies and are subject to the errors that are common to these methods (less than complete survey coverage, less than 100 percent response rates, reduced data quality due to use of mailed instruments, and use of unverified self reports).

The developed measures of educational progress and persistence are quite time-bound and, therefore, weaker than would be desired. These measures reflect not only progression and persistence in the educational system but also the timing or pace of that progression, and do not necessarily reflect ultimate educational progress or the persistence to a specific educational goal. There is also an attenuation of the relationship between length of UB participation and educational progress/persistence due to a directional constraint on the relationship. Additionally, strict statistical control was not maintained in some analyses.

Although the analytic approaches chosen are typically quite robust, they can be challenged. The choice of analysis techniques was guided by time and budget considerations as well as by the need for easily interpretable results; however, none of the techniques used are capable of definitively establishing causation. Within the nonexperimental paradigm of this study, relationships can be established, but it is improper to consider such relationships as causal, since other unconsidered or unmeasured variables may be the true causal agents.

II. OVERVIEW OF MAJOR FINDINGS

Taken jointly, the results of the follow-up study and those of the base-year study are quite consistent in suggesting UB program impact on those outcomes where it would be expected--educational expectations and educational progress and persistence. Moreover, for progress and persistence, the suggested program impact seems to be focused at the transition step between high school completion and postsecondary entry. There is no defensible indication of program effect on high school completion or on progress and persistence once entry to postsecondary education has been gained.

Suggested program impact is also typically incremental--greater impact with greater exposure to the program. The most clear cut incremental relationship is associated with educational expectations, for which each added year of program exposure provides a well-defined increase in expectations. For most of the progress and persistence measures, however, suggested incremental impact would seem to be an all-or-none function, with little or no increase as a result of one-year's participation and with a more-or-less equal increase attributable to either two-years' or three-years' participation. The incremental relationship of extent of program participation to educational progress and persistence is also seen to be focused in the area of postsecondary entry. Although the increments in educational outcomes as a function of program participation appear to be of sufficient magnitude to offset some of the decrements associated with being over age-in-grade, poverty level, academic risk, or ethnic minority, they are not sufficient to offset all such decrements.

A high degree of confidence and generality is indicated for the finding related to educational expectations, due to the basic consistency of the results obtained through two different analytic approaches (both of which controlled for preexisting differences between the groups in respect to student background factors) and the replication of the findings of the base-year study. A very obvious explanation for the greater expectations of former UB participants is that the program serves to raise the educational sights and the self-confidence in educational completion among participants; however, this is not the only explanation. Since expectations are influenced by aspirations and since no base-line measure of aspirations was obtained for individuals prior to program participation, it is possible that the participant-nonparticipant difference in expectations is at least partially attributable to preexisting motivational differences. This is an even more plausible explanation given the base-year finding that motivation for entry and completion of PSE was an important consideration for selection of participants by many of the UB projects.

For reasons similar to those given above, there is also high confidence in the findings related to overall educational progress and persistence. The nature of the difference in educational progress/persistence of former UB participants and the comparison group of nonparticipants is presented in Figure 6.1, which depicts the progress of the two groups

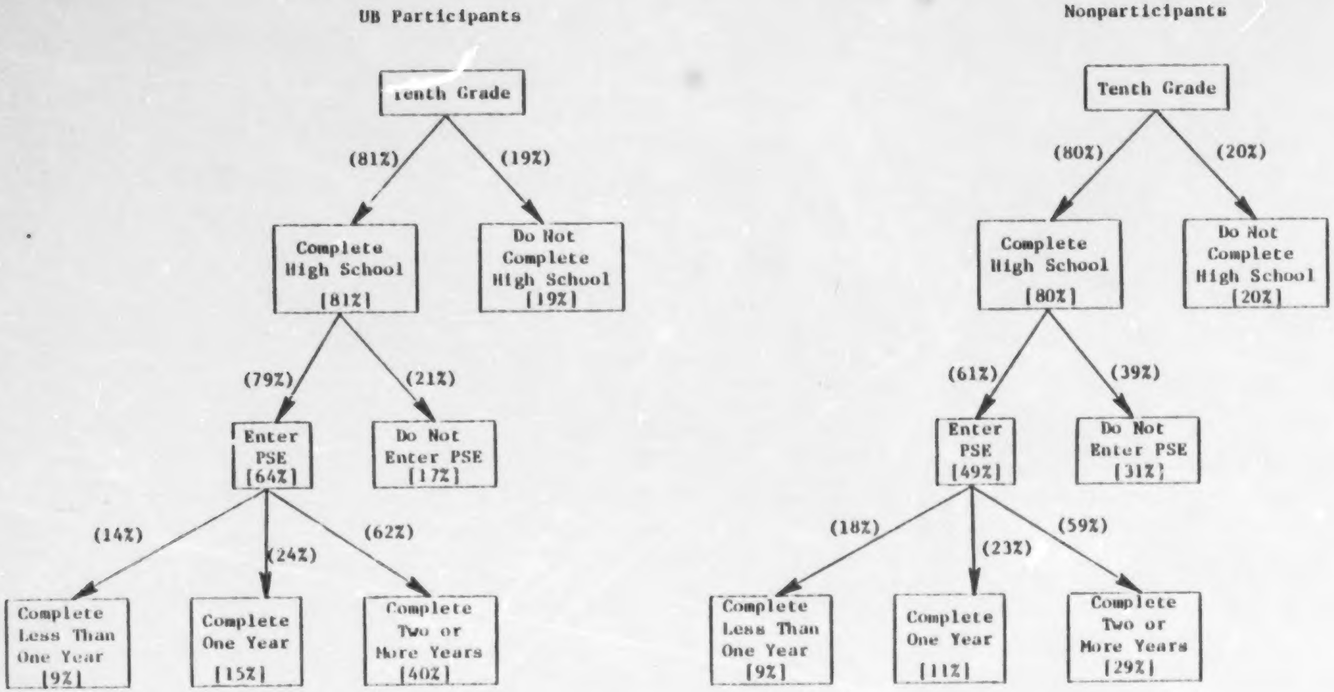
UB Participants
Tenth Grade
(81%)
(19%)
Complete High School [81%]
Do Not Complete High School [19%]
(79%)
(21%)
Enter PSE [64%]
Do Not Enter PSE [17%]
(14%)
(24%)
(62%)
Complete Less Than One Year [9%]
Complete One Year [15%]
Complete Two or More Years [40%]
Nonparticipants
Tenth Grade
(80%)
(20%)
Complete High School [80%]
Do Not Complete High School [20%]
(61%)
(39%)
Enter PSE [49%]
Do Not Enter PSE [31%]
(18%)
(23%)
(59%)
Complete Less Than One Year [9%]
Complete One Year [11%]
Complete Two or More Years [29%]

Figure 6.1. Composite of educational progress of Upward Bound participants and nonparticipants

NOTE: Figures provided within parentheses along the paths (arrows) between educational states are estimated conditional probabilities of advancement to a subsequent educational state given the attainment of a previous state. Figures provided in brackets within the boxes denoting educational states are estimated percentages of individuals in each group that attained that state. These latter percentages are the products of the conditional rates of the paths leading to a particular state.

through discrete educational steps in a composite tree diagram[1]. Figure 6.1 shows conditional rates of advancing to a subsequent educational level given attainment of a previous level (these rates are provided in parentheses along the paths) and the percentages of the original groups attaining the various levels (these percentages are the products of the conditional rates and are provided in brackets within the boxes denoting educational levels).[2] The figure clearly shows the reason for the greater overall educational progress (bracketed figures for terminal levels) of former UB participants. Specifically, the greater overall progress of former program participants is primarily attributable to the greater rates at which they enter PSE.

The greater postsecondary entry rate of former UB participants should be considered, however, in light of differences in the types of institutions entered and the types of individuals who enter. Former program participants enter four-year institutions, institutions with more attractive financial aid policies, and institutions hosting UB projects in greater relative numbers than nonparticipants in the comparison group. There is also an indication that postsecondary entry is more egalitarian for UB participants than nonparticipants, suggesting that the program may be assisting participants in overcoming some longstanding barriers to PSE. Among the postsecondary-entering subgroup of nonparticipants, males, poverty status individuals, ethnic minorities, and academic risks are underrepresented; this is not the case for former UB participants.

The specific examination of progress and persistence after PSE entry showed no advantage for former participants but was basically inconclusive. The results involve progress and persistence measures after a maximum of only three and one-half years following high school completion. It is unrealistic to expect that these measures are not deeply confounded with the individual's pace of educational advancement.

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[1] The composite was determined from two different grade cohorts. High school completion rates were obtained from tenth-grade cohort results, and subsequent educational progress rates were obtained from twelfth-grade cohort results.

[2] In interpreting Figure 6.1 it should be realized that the percentages attaining any given continuation state are relative to the specific time-span of the study, three and one-half years, and that they are, therefore, typically underestimates of rates that would be obtained over a longer period of time.

Among the population of nontraditional students being considered, temporary interruptions in educational progress and persistence toward ultimate educational goals are to be expected. While no general impact of UB program participation on postsecondary success was suggested by the current results, this in no way suggests that impact would not be indicated by more long term measures.

While it is impossible to definitively state that the greater expectations, progress, and persistence of former UB participants was brought about (or caused) by their exposure to the program, such a conclusion seems the most likely one. The pattern of results are closely alligned with what one might expect as a result of the common areas of project emphases and strategies, and the incremental nature of the outcomes for different extent of program exposure is also quite intuitive in light of program operations.

Virtually all projects emphasize the importance of postsecondary entry (particularly into four-year institutions) and assist participants in applying for and gaining both admission to some postsecondary institution and financial aid from that institution. It would be surprising indeed if these efforts were not reflected in greater entry rates of participants (particularly in four-year institutions and in those offering more attractive financial aid packages). Project efforts toward placing participants at institutions hosting UB and SSDS projects would also be expected to result in the observed greater rates of attendance of former participants at such institutions. Moreover, findings indicated that participants who were in the program during their senior year in high school (the time during which the projects place greatest emphasis on the PSE application and admission process) entered PSE at greater rates than participants who were not.

Similarly, projects uniformly provide support and counseling to the participants toward increasing self-confidence in their abilities and toward raising their educational sights. With these treatments and with the advantage realized in gaining postsecondary entry, the greater expectations of former participants are also not surprising.

Conversely, projects differ considerably in their intervention strategies in assisting the participant toward high school graduation and in preparing him for success in postsecondary education. Practices range from basic and/or remedial academic instruction through activities geared toward "general support" and/or "heightened self-esteem." Since some projects tailor their intervention strategies to the characteristics of the participants whom they select and since participants differ from site to site, the variation in project practices are neither unexpected nor unwarranted. In those areas toward which these intervention strategies of less commonality are directed, former participants, as a group, show no major differences from nonparticipants, as might also be expected.

Whatever impact the UB program may have on the outcomes of participants, suggested impacts appear to be pervasive and egalitarian. There was no indication of differential project effectiveness on any of the outcome measures considered. Moreover, both the relational analyses and the comparative descriptive analyses suggested that any benefits derived from the program by participants were more-or-less the same regardless of the individual's sex, race, poverty status, or risk status.

III. SUMMARY AND INTERPRETATION OF SPECIFIC FINDINGS

A. Noneducational Outcomes

Two noneducational outcomes were evaluated for possible relationships to UB participation: current work status and current military status. While these outcomes do not reflect directly any of the UB objectives, they are alternate activity states to current school enrollment, and serve to complete the picture of the current status of the former participant and nonparticipant groups.

There were no consistent or systematic differences between former UB participants and nonparticipants in respect to military service status or work status. Approximately 9 percent of both groups reported current active military service, and approximately 3 percent reported prior service.

As had been anticipated, current work status was a function of current enrollment; however, work status was not found to be related to UB program participation. For both former participants and nonparticipants, between 60 and 65 percent of those not currently enrolled were working full-time or part-time, while only 38 to 42 percent of those enrolled reported current employment. The lack of participant-nonparticipant work status differences for those currently enrolled suggests that former UB participants enjoy no advantage over nonparticipants in having to resort to full- or part-time employment while continuing their education.

B. Educational Expectations

A significantly larger percentage of the former participants (55 percent) expected to complete at least a four-year college education than did the nonparticipants (40 percent), and, among former participants, expectations were generally higher given longer participation in the UB program. This finding is similar to the results of the base-year study; however, while the current expectations of nonparticipants were slightly less than those reported during the base-year, the current expectations of participants were slightly greater.

The relational results allowed some elaboration of this finding, suggesting relatively straightforward relationships between various background factors and educational expectations. Being an academic risk, poverty level, or over age-in-grade were associated with lowered expectations, but prior participation in the UB program was associated with increased expectations. Even a single year of program participation was associated with a significant increase in expectations that was sufficient to offset the decrease associated with being either poverty-level or over age-in-grade. The increase in expectations associated with three years of program participation effectively offset the combined decrease associated with being an academic risk and within poverty level.

C. Educational Problems Experienced

Of six specified potential problem areas that one might experience in obtaining a desired level of education, somewhat greater proportions of former UB participants than nonparticipants reported having actually experienced difficulties due to poor high school preparation (19 versus 15 percent), family finances (38 versus 28 percent), and inadequate financial aid (28 versus 18 percent). The implication of these findings may not be as great as it first appears, when one considers the many alternate explanations. First, since former participants have higher educational expectations, it is reasonable to expect that they would perceive more problems in realizing those greater expectations. Second, as discussed subsequently, UB participants generally attend different types of PSE institutions than nonparticipants, and the various contextual factors associated with different institutional environments may reasonably be expected to influence the types of problems experienced. Finally, the base-year study suggested that considerable time is devoted to activities that could sensitize participants to the problems of poor high school preparation and inadequate finances. Therefore, the greater reported incidence of experiencing such problems among the former participants may reflect, in part, a greater awareness on their part of the many facets of these problems.

D. High School Completion

One important milestone in the educational progress and persistence of an individual is the completion of secondary school, and, while the major objectives of the UB program are related to success in postsecondary education, the completion of high school is a step in that direction and a goal that is recognized explicitly or implicitly by the various UB projects. The general thrust of the findings, which is in basic agreement with results of the base-year study, indicates that former UB participants, as a group, did not demonstrate significantly greater high school completion rates than nonparticipants.

In general, secondary completion rates were high for both participants and nonparticipants, and the rates showed an increase with higher initial grade cohort, reflecting the cumulative per-grade potential for high school dropout among those who were initially in the tenth or eleventh grade. Among the UB participants in the tenth grade during the 1973-74 academic year, 81 percent reported having completed the twelfth grade as compared to 80 percent of the nonparticipant tenth graders. In the eleventh-grade cohort, 93 percent of the former participants and 86 percent of the nonparticipants reported high school completion. The high school completion rates for both former participants and nonparticipants in the twelfth-grade cohort was 96 percent. The greater completion rate of former participants within the eleventh-grade cohort was marginally significant, but the fact that this result was not replicated in the other grade cohorts suggests that it should not be overinterpreted.

The examination of high school completion rates also indicate that secondary school completion is positively related to length of program participation; however, this finding is somewhat artifactual, since the relationship is directionally constrained (when a UB participant drops out of high school, he is terminated from the program). Supplementary analyses provided empirical confirmation of the artificial nature of this finding for the tenth- and eleventh-grade cohorts. Still, the finding within the twelfth-grade cohort is relatively free of this relational constraint and indicates that those who join the program in the twelfth grade complete high school in relatively smaller numbers (90 percent) than those who have participated in the program prior to the twelfth grade (98 percent). These results suggest that the twelfth-grade-only participants (who probably represent replacements for prior participant attrition) are somewhat different from the modal participants. With the absence of defensible replication in other grade cohorts, additional interpretation of the relationship would be speculative.

E. Postsecondary Entry

An obvious requisite for success in education beyond high school is gaining admission to a postsecondary institution. The relationship of UB participation to PSE entry is, therefore, an important consideration in investigating program impact. Findings presented below relate to three separate aspects of PSE entry: (1) entry rates, (2) types of individuals entering PSE, and (3) types of PSE institutions entered.

1. Entry Rates

For each of the three grade cohorts, the overall PSE entry rate for former UB participants was greater, by almost 20 percentage points, than that for nonparticipants. For nonparticipants, entry rates were 39 percent, 50 percent, and 58 percent for the tenth- through twelfth-grade cohorts; respectively. Analogous rates for former UB participants were 55 percent, 68 percent, and 76 percent. The rates also suggest a differential likelihood of PSE entry as a function of grade cohort considered. Some of this differential is no more than a reflection of cohort differences in likelihood of high school completion; however, the differential should also reflect the additional time for delayed PSE entry (atypical educational progression) for those in the higher grades initially.

To obtain some estimate of the magnitude of delayed entry and to provide statistics free of the directional constraints on the relationship of extent of program participation to high school completion, a conditional PSE entry rate was also computed. This rate indicated the likelihood of PSE entry, given high school completion. Although these conditional rates were necessarily higher than the overall rates, the two rates were in complete agreement in terms of the differential between former participants and nonparticipants. The conditional PSE entry rates were most effective in demonstrating that longer participation in the UB program is associated with greater PSE entry rates. This trend was evidenced for each of the three grade cohorts and was in all cases statistically significant. In the aggregate, PSE entry rates were 63 percent, 75 percent, and 82 percent, respectively, for those with one, two, or three years' participation in the program.

The conditional rates also increase over grade cohort, suggesting that delayed PSE entry may be an important factor in establishing an ultimate rate of entry for a particular age group. A more direct examination of the incidence of delayed PSE entry was also conducted. This examination indicated that substantial percentages of those individuals who did not enter PSE during the base-year subsequently entered PSE after a delay. This held for both nonparticipants (36 percent) and former participants (62 percent), for whom the delayed entry rate was clearly greater. This finding, coupled with base-year results indicates that not only do UB participants enter PSE at greater rates than nonparticipants immediately following high school, but also the subsequent rate of lag entry (among those who do not enter immediately) is greater among UB participants.

The relational analyses supported these results and indicated straightforward relationships of student background characteristics, temporal factors, and extent of program participation to the likelihood of PSE entry . In addition to the decrease in likelihood of PSE entry as a result of being in a lower initial grade cohort, a decrease in expected PSE entry was indicated as a function of being over age-in-grade, of an ethnic minority other than Black, or an academic risk. UB participation was associated with an increase in the likelihood of PSE entry. One year of program participation provided an insignificant increase in predicted PSE entry rate, but two or more years of program participation was associated with a significant increase in the likelihood of PSE entry. Two years of participation in the program offset the decrease in likelihood of PSE entry associated with non-Black minority status, and three years' exposure to the program offset the combined decrease in likelihood of PSE entry associated with being an academic risk and being one year over age-in-grade.

2. Types of Individuals Entering Postsecondary Education

The types of former participants entering PSE were different from those entering from the adjusted group of nonparticipants. Among the PSE- entering subgroup of UB participants,the proportions of ethnic minorities, poverty status individuals, and academic risks coincided with the analogous proportions in the total participant population. Among the PSE-entering subgroup of nonparticipants, all such groups were underrepresented. These results provided considerably greater substance to the results of the relational analyses reported in the previous subsection, suggesting that the statistical compensations in the regression equation are, in fact, reflected in a greater likelihood of PSE entry for nontraditional students who have participated in the UB program.

3. Types of Postsecondary Institutions Entered

The types of postsecondary schools attended by former participants differed from those attended by nonparticipants. A greater percentage of former UB participants (66 percent) reported attendance at four-year institutions or universities than did nonparticipants (51 percent), which is directionally consistent with results of the base-year study. Former participants also report greater attendance at institutions hosting an Upward Bound project (47 percent) than nonparticipants (23 percent). Attendance at SSDS host institutions was also slightly, but not significantly, greater among former participants than among nonparticipants. The results also suggest that UB participants attend the traditionally Black institutions at greater rates than nonparticipants (although this is probably related to their greater attendance rates at UB host institutions) and that they attend institutions with more attractive financial aid policies at somewhat greater rates than nonparticipants.

The considerably greater rate at which former participants attend UB host institutions is an interesting but not unexpected finding. The differential attendance rates at these institutions cannot be explained by proximity since the nonparticipant sample was drawn from the same locations as the participant sample. Host institutions are committed to the program and to serving its participants, and this finding certainly suggests that enrollment of former participants in host institutions is disproportionately large relative both to the nonparticipant enrollment and to the proportion of host institutions in the institution population. It is suspected that at least part of this difference is attributable to the fact that a farily large number of UB participants are admitted to the particular institution that served as host for the specific UB project in which they participated.

F. Postsecondary Persistence and Progress

The legislatively mandated objective of the UB program is to provide the participants with the "skills and motivations necessary for success in education beyond high school;" therefore, examination of postsecondary success, as reflected in the measures of persistence and progress developed in this study, was an important consideration. The investigation was restricted to the subsets of former UB participants and nonparticipants who entered some form of postsecondary schooling, and, from the findings already presented, it is obvious that completely clear-cut results could not have been expected. The former participants entering PSE differed from PSE-entering nonparticipants. The educational expectations of the former participants were greater, they entered different kinds of postsecondary institutions, and they reported having experienced more problems in fulfilling their educational aspirations. Moreover, the available measures of postsecondary progress and persistence were particularly time-bound. In light of these limitations, these findings must be interpreted cautiously.

Analyses were unable to detect any consistent or systematic difference between former UB participants and nonparticipants in respect to postsecondary progress, as measured by highest grade completed. The one suggestion of a relationship between program participation and PSE progress was for the eleventh-grade cohort, in which former participants held a marginally significant advantage. No differences in PSE progress were observed within the participant group as a function of length of program participation.

Analyses did detect differences in the current enrollment rates of former participants and nonparticipants. Nonparticipants who had entered PSE were currently enrolled at higher rates (76 percent) than were former participants (67 percent). This finding for the aggregate group was primarily attributable to the difference observed within the twelfth-grade cohort. In general, current enrollment rates tended to decrease from the tenth- to the twelfth-grade cohort within both the participant and nonparticipant groups. More than likely this reflects a cumulative attrition within the higher grade cohorts, who, under typical progression, would have been enrolled in PSE for a longer time. No differences in current enrollment were observed within the participant group as a function of length of program participation.

These findings indicate that while former UB program participants have progressed as far in postsecondary education as nonparticipants they are currently enrolled in smaller relative numbers. The fact that participants demonstrated slightly greater progress than nonparticipants in the eleventh-grade cohort and lower current enrollment than nonparticipants in the twelfth-grade cohort suggests that the attrition among former participants is greater than that of nonparticipants following the second year of PSE but that through the second year participants are progressing as well or better than nonparticipants.

It is reasonable to expect that the postsecondary progress and persistence of individuals would vary as a function of the type of PSE institution they attend and the availability and adequacy of financial aid. It is also conceivable that differences in postsecondary progress and persistence between former UB participants and nonparticipants might be modified when one considers these factors. Such questions were examined empirically by considering three institutional characteristics (level of offering, UB host status, and SSDS host status) and the sample members stated experience of educational problems as a result of inadequate financial aid.

No systematic or meaningful differences in educational progress as a function of institution type or of perceived adequacy of financial aid were uncovered; however, this investigation did augment the findings related to current enrollment. The disparity between current enrollment of former participants and nonparticipants was concentrated within the universities, two-year schools, and UB or SSDS host institutions as well as among the subgroup of individuals reporting problems with adequacy of financial aid. The higher current enrollment rates of twelfth-grade cohort nonparticipants within two-year institutions is seen as having no particular significance, since 1973 twelfth-graders should have completed a two-year course of study by the time of the follow-up study. The other areas in which disparities exist suggest a complex pattern that resists an immediate or simple explanation, particularly in light of other findings.

G. Overall Educational Progress

The results reported in the previous subsections indicate educational progress and persistence for particular segments of the educational ladder; however, taken singly they do not present an integrated picture of educational progress. The overall educational progress of former UB participants is greater than that of the nonparticipants, and, within the participant group, overall educational progress is greater with longer participation in the program. This finding held within each of the three grade cohorts considered; however, within the tenth- and eleventh-grade cohorts, the finding related to length of program participation was attenuated by the directional constraint on the relationship. The nature of this difference in overall educational progress has been presented in Figure 6.1, which demonstrated that the greater overall progress of former program participants is primarily attributable to the greater rates at which they enter PSE.

The relational analyses provided additional support for these results and indicated that the predicted overall educational progress of an individual was a relatively straightforward function of background characteristics, temporal factors (age and grade cohort), and extent of UB program participation. In addition to the obvious limitations on educational progress imposed by grade cohort, the relational analyses indicated that being an academic risk or over age-in-grade was associated with less educational progress. Participation in the UB program, on the other hand, was associated with greater educational progress. The increase in expected educational progress associated with one year of program participation was not significant. The increase associated with two years of participation was only slightly less than that associated with three years of participation, either of which was sufficient to offset almost completely the decrease associated with being an academic risk.

H. Additional Implications from Relational Analyses

In general, the relational results supported and augmented the comparative descriptive results in respect to overall educational progress, PSE entry, and educational expectations. These findings have been presented above. Additionally, some findings of the relational analyses provided additional insight into the nature of suggested program impact.

The most surprising finding was that the relationships between the three outcome measures and the variables used to predict them were quite stable regardless of the particular project site involved or the particular characteristics of the sample member (i.e., none of the relationships were interactive). In other words, once the complex of predictor variables (sex, race, age, risk status, poverty status, grade, extent of program participation, and specific project association) was considered, any additional level of explanation of outcome measures afforded by considering interactive relationships was effectively nonexistent. A particularly relevant implication from this finding is that any suggested impact on these intermediate educational outcomes attributable to UB participation is effectively the same for Blacks, whites, or other ethnic groups; for males or females; for poverty level and nonpoverty individuals; for academic risks as well as nonrisks; for tenth, eleventh, or twelfth graders; and for any project site considered.

The lack of an interactive relationship between project association and extent of participation in the UB program is of particular importance since it implies that differential project effectiveness can be discounted in explaining variation in the outcome measures selected. That is, when student characteristics, temporal factors, contextual factors, and length of program participation are controlled, only negligible differences remain between projects in the selected outcome measures.

This finding supports the results of the base-year study, which indicated that projects differ in types of operation, types of students, and relative success in certain outcome measures but that when one controls for student input characteristics, project outcome differentials are negligible.

The relational analyses also revealed that the contextual (or site) factor (i.e., different geographical areas as represented by specific clusters of schools) did not demonstrate any appreciable added explanatory power in accounting for variability of any of the three educational outcome measures. That is, *any variation of educational progress, persistence or expectations as a function of site is equally explainable by differences in either individual characteristics, temporal factors, or length of program participation among those sites*. The finding that variation between sites is small relative to variation of individuals within sites is a fairly common finding in educational research.

Considerable internal stability of the various analyses was also suggested by the fact that the solutions for the prediction equations were quite similar for each specific outcome considered. For each of the outcomes the regression weight associated with a specific predictor variable was consistently in the same direction and of a similar relative magnitude as analogous regression weights for other outcomes.

Appendix A

Sampling Methodology and Sampling Error Computation

Appendix A

Sampling Methodology and Sampling Error Computation

I. SAMPLING OVERVIEW

A major purpose of the national evaluation study was to estimate the effect of the Upward Bound (UB) program on the educational continuance rates of the high school students it serves. In order to estimate this effect, it was necessary to have some standard against which to compare the continuance rates of the UB participants. The study was, therefore, also concerned with measuring continuance rates among nonparticipating students (comparison students) who were similar enough to the UB participants to provide meaningful benchmarks against which the rates of participants could be compared. The study measured the rates for (1) UB participants who were high school sophomores, juniors, or seniors at the beginning of the 1973-74 school year and (2) comparison students in these same three grades at that time. It should be kept in mind that the UB program was evaluated after it had been operational for a number of years and that the evaluation procedures available at such a stage do not provide the precise answers that would be forthcoming from an experimental design.

Since statistical procedures exist to obtain unbiased estimates of population characteristics from data based on only a sample from that population, a sample survey procedure was used, and a probability sample of UB participants and comparison students was drawn. Estimates of sampling error can also be obtained from such a probability sample. The sampling error (or standard error) provides a measure of the range within which a sample estimate can be expected to fall a certain proportion of the time. For example, if the tenth-grade dropout rate in the population of UB participants was ten percent, and if it was calculated that a sample of a given size selected in a prescribed way would yield a sampling error of 1 percent, then, by invoking the Central Limit Theorem, two-thirds of all possible samples of that same size selected in that same way would yield retention rate estimates between 9 percent and 11 percent, while 95 percent of such samples would yield estimates between 8 percent and 12 percent. Stated somewhat differently, the likelihood that an interval of 2 percentage points on either side of the sample estimate will contain the true population rate is 95 percent. The magnitude of sampling error is related to two things over which some control can be exerted: the size of the sample, and the procedures used in selecting the sample.

In developing a sample design, concern is given to selecting a sample that will yield estimates of sufficient precision (i.e., estimates having a small enough sampling error so as to be useful) and to producing these estimates at the least cost. In most instances, a sample that is widely spread over the entire population will produce the most precise estimates. However, the cost of conducting a study with such a widespread sample is usually much greater than the cost of a study using a cluster sample (i.e., a sample in which several sites are selected, following which several individuals are selected from the same site). By balancing both expected precision and expected costs, the most desirable sample design can be determined. For the UB evaluation study, rather than selecting a sample of UB participants without regard to their location, a two-stage sample design was planned. The first stage involved the selection of a sample of UB projects, the second stage involved the selection of a sample of tenth-, eleventh-, and twelfth-grade students who were participating in those UB projects selected in the first stage.

Since the purpose of the study necessitated the estimation of continuation rates not only for UB students but also for a comparison group of similar students, a comparison population had to be defined and sampled. It was decided that a group satisfying the requirements of similarity and comparability to the UB students was the population of nonparticipating students attending the same schools as the UB students (subsequently referred to as feeder schools). This definition of a comparison group had both analytical and administrative advantages.[1]

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[1] By using as control students those who attended the same schools as UB students, one controls for any differential school effects. This can be expected to have the effect of increasing the precision of estimates of differences between UB students and control students. In addition, using such schools provides a geographic clustering of the UB and comparison student samples and an associated reduction in cost.

The proposed definition of comparison students has one obvious disadvantage. To the extent that UB activity in a school has beneficially affected students who otherwise are not associated with the UB program, the study results would be biased in the direction of underestimating the beneficial effect of the UB program on educational continuance rates.[2] All things considered, however, the proposed definition was judged to be the most desirable.

Sample optimization procedures suggested that 54 UB projects would be selected for the sample. For each sample project, all students in grades 10, 11, and 12 who were in high school and who participated in that project during September or October 1973 were included in the sample. In addition, for each sample project two feeder schools were selected, and from each feeder school seven or eight comparison students were taken from each of grades 10, 11, and 12. To further control the amount of field work involved in the sampling procedures, it was decided to sample students within the selected schools in two stages, by first selecting a sample of classes and then selecting a sample of students within the selected classes. A graphic representation of student sampling, showing the different stages of selection is presented in Figure A.1. A more detailed description of the selection of the 54 sample projects and of the subsequent stages in the selection of the student samples is presented below in Section II.

Other samples were drawn in the base-year study (namely, samples of project and high school staff and a subsample of projects for site visits). These sampling procedures are not discussed here since they do not pertain to the follow-up study. The interested reader can refer to Appendix B of the base-year study[3] for details regarding these additional samples.

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[2] Another disadvantage associated with the proposed group of comparison students is that they might differ from UB students on important background characteristics. Appendix E details the techniques used to adjust for such differences.

[3] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-153)

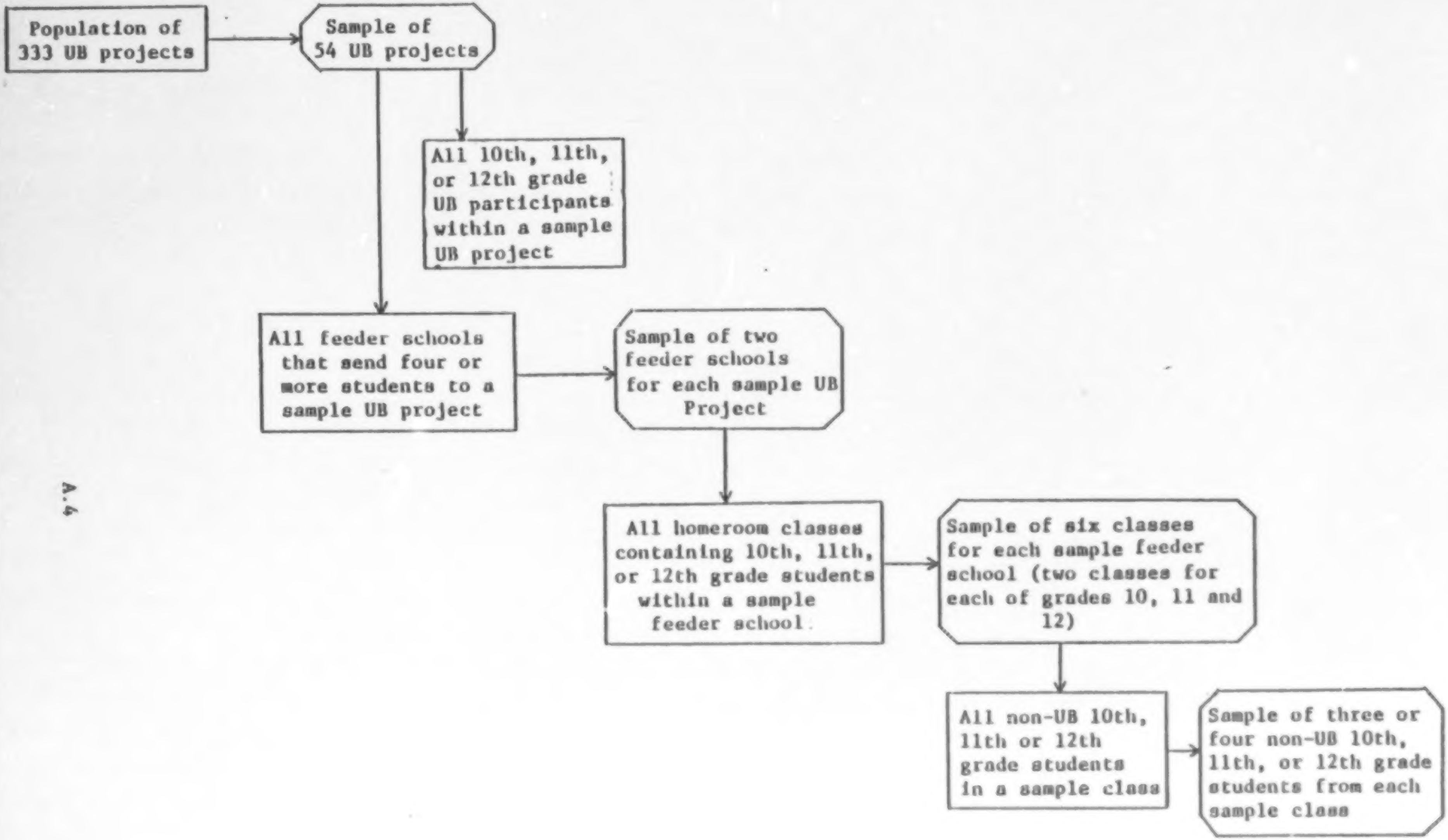
Population of 333 UB projects
Sample of 54 UB projects
All 10th, 11th, or 12th grade UB participants within a sample UB project
All feeder schools that send four or more students to a sample UB project
Sample of two feeder schools for each sample UB Project
All homeroom classes containing 10th, 11th, or 12th grade students within a sample feeder school
Sample of six classes for each sample feeder school (two classes for each of grades 10, 11 and 12)
All non-UB 10th, 11th or 12th grade students in a sample class
Sample of three or four non-UB 10th, 11th, or 12th grade students from each sample class

Figure A.1. Structure of the UB Student and Control Student Samples.

II. SPECIFIC SAMPLING PROCEDURES FOR BASE-YEAR STUDY[4]

A. The UB Project Sample

RTI obtained from the U.S. Office of Education (USOE) and from the 10 Regional USOE offices copies of the project proposals for all regular[5] UB projects that were funded for fiscal year 1973-74. From these proposals characteristics of the funded projects were obtained, and a frame from which projects would be sampled was constructed. To have been included in the project sampling frame, a project must have possessed all of the following characteristics:

1) The project was funded for fiscal year 1973.
2) The project proposed to serve students in grades 10, 11, and/or 12.
3) The project was located in coterminous United States.

The 333 projects possessing all of the above characteristics constituted the frame from which sample projects were selected. Using project characteristics obtained from the project proposals, the 333 projects were partitioned into 27 strata on the basis of 6 variables. These variables, together with their category levels, are listed below:

1) Ethnic background of participants with six levels:[6] (a) Black, (b) Black and White, (c) 10+ percent American Indian, (d) 10+ percent Mexican American, (3) 10+ percent Puerto Rican, and (f) all other.
2) Office of Education geographic region with 10 levels: (regions 1 through 10).

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[4] For the reader desiring more detailed discussion of the sampling techniques described in this appendix, see Kish, L., Survey sampling, New York: John Wiley & Sons, 1965.

[5] Special projects for veterans and demonstration projects were excluded.

[6] Ethnic Composition Code, Based on Racial Distribution of Proposed Participants for project year 1973-74. Black: All or mostly black, i.e., no other race $\geq$ 10 percent. Black and White: Black $\geq$ 10 percent and White $\geq$ 10 percent and no other race $\geq$ 10 percent. American Indian: American Indian $\geq$ 10 percent. Mexican American: Mexican American $\geq$ 10 percent and American Indian $\leq$ 10 percent. Puerto Rican: Puerto Rican $\geq$ 10 percent and American Indian $\leq$ 10 percent and Mexican American $\leq$ 10 percent. Other: All other.

3) Program emphasis, with four levels: (a) academic orientation, (b) personal development orientation, (c) combination of academic and personal development orientation, and (d) other.

4) Project age, with four levels: (a) new this year, (b) 1 year old, (c) 2 years old, and (d) 3 or more years old.

5) Location of participants, with seven levels: (a) citywide, (b) selected parts of a city, (c) rural, (d) city and rural, (e) statewide, (f) reservation, and (g) regional.

6) Type of sponsoring institution or agency, with nine levels: (a) secondary school, (b) two-year college, (c) four-year college, (d) vocational-technical school, (e) private, nonprofit agency, (f) consortium of educational institutions, (g) agency for a consortium, (h) proprietary school, and (i) public agency.

The strata were constructed so as to have approximately equal total numbers of participants in grades 10, 11, and 12. From each of the 27 approximately equal-size strata, two different projects were selected for the sample with probability proportional to a measure of size, which reflected the total number of participants in grades 10, 11, and 12.[7] This resulted in the selection of 54 different UB projects.

A description of the 27 strata that were formed is provided in Table A.1. As evident from the descriptions of the 27 strata, not all variables were used in defining each separate stratum. Stratum number 1, for example, consists of all projects in which the ethnic composition of the clients was coded as "Black" and which are located in USOE geographic region number 5. The "all" listed in the last four columns of the row labeled stratum number 1 indicates that these variables were not used in classifying projects into this stratum, i.e., any level of these variables was permissible.

It may be useful to consider the stratification and selection of the 54 sample projects in more detail. Stratification may be defined as the division of a population into subparts called strata, for the purpose

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[7] The procedure used is described in detail in M. R. Samford, On sampling without replacement with unequal probabilities of selection, Biometrika, 54, 4, 1967, pp. 499-512.

Table A.1

DESCRIPTION OF THE 27 STRATA FOR SELECTING PROJECTS

Stratum Number	Characteristics of Projects in Stratum					
	Ethnic Composition[a]	Region	Program Emphasis	Project Age	Location of Participants	Sponsor
1	Black	5	All	All	All	All
2	Black	3	All	All	All	All
3	Black	1,2	All	All	All	All
	Black	4,6	All	Under 2 years	All	All
4	Black	4,6	All	2 years	All	All
5	Black	4,6	All	3+ years	All	All
6	Black & White	3	Academic	All	All	All
7	Black & White	4	Academic	3+ years	Not all city	4-year college
8	Black & White	6	Academic	All	All	All
	Black & White	4	Academic	Under 3 years	All	All
	Black & White	4	Academic	3+ years	City	All
	Black & White	4	Academic	3+ years	Not all city	Other than 4-year college
9	Black & White	3,4,6	Personal Development	All	All	All
10	Black & White	3,4,6	Combination	3+ year	All	All
11	Black & White	3,4,6	Combination	Under 3 years	All	All
12	Black & White	5	All	All	Not regional	All
13	Black & White	1,2,7	All	All	All	All
		5	All	All	Regional	All
14	10%+ American Indian	All	All	All	Reservation	All

Table A.1 (continued)

Stratum Number	Characteristics of Projects in Stratum					
	Ethnic Composition[a]	Region	Program Emphasis	Project Age	Location of Participants	Sponsor
15	10%+ American Indian	8,9,10	Academic	All	Non-Reservation	All
16	10%+ American Indian	1,2,3 4,5,6 7	Academic Skills	All	Non-Reservation	All
17	10%+ American Indian	All	Other than academic skills only	Under 3 years	Non-Reservation	All
18	10%+ American Indian	5,7,8 9,10	Other than academic skills only	3+ years	Non-Reservation	All
19	10%+ American Indian	1,2,3 4,6	Other than academic skills only	3+ years	Non-Reservation	All
20	10%+ Mexican American	3,4,6	All	All	City or city parts	All
21	10%+ Mexican American	3,4,6	All	All	Not all city	All
22	10%+ Mexican	1,2,5 7,8,9 10	Academic Skills	All	All	All
23	10%+ Mexican American	1,2,5 7,8,9 10	Other than academic skills only	All	All	All
24	10%+ Puerto Rican	All	All	All	Not all city	All
25	10%+ Puerto Rican	All	All	3+ years	City or city parts	All
26	10%+ Puerto	All	All	Under 3 years	City or city parts	All
27	Other	All	All	All	All	All

[a] Ethnic Composition Code, Based on Racial Distribution of Proposed Participants for project year 1973-74
Black: All or mostly black, i.e., no other race ≥10%.
Black and White: Black ≥ 10% and White ≥ 10% and no other race ≥ 10%.
American Indian: American Indian ≥ 10%.
Mexican American: Mexican American >10% and American Indian < 10%.
Puerto Rican: Puerto Rican ≥10% and American Indian ≤10% and Mexican American ≤10%.
Other: All other.

of sampling separately from each stratum. Although there are a number of reasons for stratifying prior to the selection of the sample, the 27 strata were formed for the selection of 54 sample Upward Bound projects for two basic reasons: (1) to insure against underrepresentation of certain minority groups (e.g., American Indian, Mexican American, and Puerto Rican) in the sample; and (2) to attempt to reduce the size of sampling errors that otherwise would result if a sample of 54 projects were selected without stratification.

In forming the 27 strata, the aim was to make the projects within each stratum as similar as possible on the variables of central interest in the study (e.g., educational continuance rate for UB participants) and to make the 27 strata as different from one another as possible on these variables. To the extent that this is done successfully, the sampling errors will be decreased, as compared to those based on an unstratified design. Ideally one would form strata on the basis of the central variables themselves, but, because such information is generally not available, the strata are usually formed on the basis of available characteristics thought to be correlated with the central variables of the study. To the extent that stratifying on these related characteristics actually does form strata that are homogeneous with respect to the central variables under study, the sampling errors of the estimates of these variables will be reduced.

The stratification variables used in dividing the 333 projects into 27 strata were those thought to be related to educational continuance: ethnic background, geographic region, UB project program-emphasis, project age, location of participants, and project sponsor. If all of these six variables were cross-classified with one another, there would be some 108,000 cells. Since there were only 333 projects, most of the cells would have been empty and many would have contained only one or two projects. It is obvious that considerable combining of cells was necessary. In attempting to combine cells to form homogeneous groups, some "miscellaneous" strata resulted (e.g., stratum number 8-- see Table A.1). In addition, the combination process resulted in certain categorizations being used for some groups and not for others. For example, city versus noncity was a meaningful and useful way to divide projects having a sizeable number of Puerto Ricans, but was not useful in dividing projects classified as American Indian; for this latter group, a reservation versus nonreservation distinction was made.

In forming the homogeneous strata, an attempt was made to form the strata so that they were as nearly equal in size as possible (i.e., equal with respect to the number of UB participants anticipated for the school year 1973-74). This was done because using equal size strata generally can be expected to bring about greater gains in precision of estimates relating to student characteristics than using strata that vary greatly in size. This set of 27 strata, of course, is only one set from among the many possible sets of 27 strata that could have been formed. It is entirely possible that another set would have been better (i.e., would have yielded estimates having a somewhat smaller sampling error than estimates from a sample based on the 27 strata that were actually used). Undoubtedly many other sets would have been worse. Different ways of forming 27 strata can affect the size of the sampling errors of the estimates, but the unbiasedness of the estimates that can be made from the study data is in no way affected. Unbiased estimates of population characteristics are possible regardless of the effectiveness of the stratification.

Once the 27 strata were formed, 54 projects were randomly selected without replacement, two from each stratum, using probabilities proportional to a measure of project size. This procedure was used in order to allow the desired control over the number of UB participants selected from each project while permitting an overall similar probability of selection of UB participants. By selecting two projects from each stratum, it is possible to make unbiased estimates of between-strata and within-strata components of variance (see Section IV of this Appendix), permitting estimation of the extent of reduction in sampling error that was brought about due to stratification of projects prior to selection.

The distribution of UB projects, both nationally and for the sample, in terms of the stratification variables are given in Table A.2. Because the projects were selected with other than equal probabilities, an unweighted distribution of sample projects would not necessarily be similar to the distribution of the population of 333 projects, except on

Table A.2

POPULATION AND SAMPLE DISTRIBUTIONS OF UB PROJECTS BY ETHNIC COMPOSITION, REGION, PROJECT PROGRAM EMPHASIS, PROJECT AGE, LOCATION, SPONSOR, AND SIZE OF PROJECT

a. Ethnic Composition

Ethnic Composition	Population	Sample	
	Percent	Percent	Number of Projects
1. Black	18	19	10
2. Black & White	33	31	17
3. Indian	20	20	11
4. Mexican	17	15	8
5. Puerto Rican	9	11	6
6. Other	3	4	2
Total	100	100	54

b. Region

Region	Population	Sample	
	Percent	Percent	Number of Projects
1	8	9	5
2	9	9	5
3	12	9	5
4	22	26	14
5	13	15	8
6	13	11	6
7	7	4	2
8	5	6	3
9	8	7	4
10	4	4	2
Total	100	100	54

Table A.2 (continued)

c. Project Program Emphasis

Program Emphasis	Population	Sample	
	Percent	Percent	Number of Projects
Academic Skills	52	48	26
Personal Development	13	11	6
Combination	30	37	20
Other	1	0	0
Not applicable (new project)	4	4	2
Total	100	100	54

d. Project Age

Project Age	Population	Sample	
	Percent	Percent	Number of Projects
New	14	7	4
1 Year	10	11	6
2 Years	12	13	7
3 Years	60	65	35
Not applicable	4	4	2
Total	100	100	54

Table A.2 (continued)

e. Location

Location	Population	Sample	
	Percent	Percent	Number of Projects
City	14	11	6
City Parts	16	17	9
Rural	9	9	5
City Rural	49	52	28
Statewide	2	2	1
Reservation	3	4	2
Regional	6	6	3
Other	1	0	0
Total	100	100	54

f. Sponsor

Type of Institution	Population	Sample	
	Percent	Percent	Number of Projects
Secondary School	2	0	0
Two-Year College	8	11	6
Four-Year College	84	85	46
Vocational/Technical School	2	0	0
Private Non-Profit	1	2	1
Combination	3	2	1
Agency	0	0	0
Proprietary School	0	0	0
Public Agency	0	0	0
Total	100	100	54

Table A.2 (continued)

Expected Number of Students in Project	Population	Sample	
	Percent	Percent	Number of Projects
0-50	19	13	7
51-75	40	35	19
76-100	23	19	10
101-125	11	13	7
126-150	4	9	5
151-175	1	7	4
176-200	1	4	2
201+	1	0	0
Total	100	100	54

those variables used for stratification. With one exception, the percentage distributions presented in Table A.2 show that the unweighted distributions of the 54 sample projects on the six variables involved in the stratification process were quite similar to the distributions based on the 333 projects in the population. The major distributional difference, as would be expected, was in respect to the variable "expected number of students in project." This difference was largely due to the fact that projects were selected into the sample with probabilities proportional to measures of project size, which gave projects having over 100 anticipated participants a greater chance of selection than projects having 100 or fewer anticipated participants.

B. The Student Samples

1. The UB Student Sample

Within each of the 54 sample projects, all students who were in grades 10 through 12 and who were UB participants in September or October of 1973 were selected into the sample. This resulted in the selection of 3,710 eligible UB students associated with the 54 sample UB projects.

2. The Comparison Student Sample

A sample of control or comparison students was selected against which to compare the UB students. These students were selected from feeder schools associated with the 54 sample projects. To adequately describe the comparison student sample, it is necessary to first define the feeder school sampling procedure.

A feeder school was defined as a school with at least four tenth-, eleventh-, and/or twelfth-grade students listed on the most recent CUB[8] roster as being clients of a given UB project, or as a school from which a newly funded UB project planned to serve at least four tenth-, eleventh-, and/or twelfth-grade students during the 1973-74 school year. For sampling purposes, each feeder school was associated with one, and only one, UB project. If a feeder school provided students to more than one project, it was associated only with that project to which it provided the greatest number of students. In order to accomplish this procedure of uniquely associating a feeder school with one and only one UB project, the CUB roster which listed the schools attended by UB students was checked for all 333 projects in the defined population.

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[8] The Current Upward Bound Student Roster as of August 1973, produced through the UB data bank.

Once the feeder schools for a sample project had been identified, they were ordered according to the number of UB students participating in the associated UB project who attended the school. The estimated total number of students in grades 10 through 12 was then recorded for each feeder school and these numbers accumulated. A systematic random selection of four feeder schools was made from the ordered list with probability proportional to size, and an equal probability subselection of two of the four schools (designated "sample" schools) was made. The remaining two schools were designated as "backup" schools to be used only if the "sample" schools refused to participate in the study. For some projects additional feeder schools were selected. This was done to keep the comparison student weights from being excessively large.

A total of 113 school selections were made, bringing into the sample 108 different schools.[9] Of the 108 sample schools selected, 5 refused to participate. Four "backup" schools (one of which was a backup for two refusals) were selected as replacements, which brought the total to 107 schools that were expected to participate. The late refusal of one school resulted in a final sample of 106 schools providing the sample comparison students that were used in the study.

Within each sample feeder school, six homeroom classes (two per grade) of students in grades 10 through 12 were selected with equal probability from class lists supplied by the sample schools. For each selected class, a list of students who were in the class during September 1973 was obtained. For each student listed, the homeroom teacher was asked to indicate the student's grade level and ethnic background, and to make an educated guess as to whether or not the student came from a low income family and as to whether or not the student should be considered as an "academic risk" for a two- or four-year college education. This information was used to stratify students prior to selection.[10] The student lists for the sample classes were carefully checked against the September 1973 list of UB students, and the students who were listed as being associated with a UB project were removed from the student class lists. From these corrected lists, a stratified random sample of comparison students was then selected, with an expected 21 students from each sample control school (42 if the school had been selected twice), yielding a total of 2,340 eligible comparison students.

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[9] The selection procedures permitted a school to be selected more than once, and in five instances a school was selected twice. In such cases, the number of sample students to be selected from the school was doubled.

The selection of the students within a school involved a predetermined set of procedures. First, using the information provided by the homeroom teacher, the students were stratified according to their grade in school and according to whether or not they appeared to meet the eligibility requirements of the UB program. Then, a random sample of students was selected from each stratum, with the objective of selecting approximately equal numbers of students from each of the three grades and of over-sampling students who appeared to be like UB students.[11] Of the students thus selected into the sample, some were tentatively classified as "like UB" and some as "unlike UB" students, but a disproportionately high number of "like-UB" students were selected. This was done in order to attempt to include a substantial number of students who were, in fact, eligible for a UB program.[12]

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[10] Rather than sending class lists, several sample schools sent complete rosters of all of their students who had been enrolled in September 1973. In such cases a sample of 20 students from each grade level was selected, and the lists of selected students were then sent back to the school with the request that the school provide information on the background characteristics of each student listed. Such completed lists were then treated as though they were class lists, for the purposes of selecting the sample comparison students for the study.

[11] In the analysis of the student data, the characteristics of comparison students were, of course, determined from different factual information rather than the teachers' educated guesses used in the sample selection procedures.

The specific procedures used in selecting the sample of comparison students involved the formation of three strata within each of the three grades. Stratum I included all students classified by the school as coming from a low income family and also classified by the school as an "academic risk." Stratum II contained all other students classified as low income. Stratum III included all the remaining students. An adjusted measure of size was calculated by multiplying the actual number of students in Stratum I by 4, the actual number of students in Stratum II by 2, and the actual number of students in Stratum III by 1. Thus, "low-income-high-academic-risk" students were selected at approximately four times the base rate and other low income students were selected at approximately twice the base rate. This measure of size adjustment was employed to increase the probability of selecting students who appeared to be like UB students. The measures of size were then totaled for the selected classes within each grade. The sample of approximately seven students[13] selected from each grade was allocated to each of the three student strata in proportion to the measures of size calculated above with one constraint--at least three students were selected from Stratum I, if the stratum contained at least three. This departure from the strict proportional allocation insures that about half the comparison sample students selected would be those thought to be "like" UB students.[14]

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[12] The samples of UB and comparison students were designed and selected in such a way as to maximize the precision of estimates of the UB population and of the comparison student population (i.e., the population of non-UB students attending feeder schools associated with the 333 UB projects). In addition, the design attempted to permit efficient comparisons of the two groups of students. The comparison group, however, was not necessarily expected to be similar to UB students on all relevant background characteristics. In order to reduce or eliminate the effects of these dissimilarities, a balancing or standardizing statistical technique was planned and ultimately employed. This technique, which is described in detail in Appendix E, statistically adjusted the comparison student population to the UB population. The oversampling of comparison students who were thought to be "like" UB students aimed at increasing the precision of the comparisons between UB students and the "balanced" or standardized comparison student population.

[13] In four schools, 14 students were selected from each grade. In one school, 10 students were selected from each grade.

At each stage of the sample selection, each unit in the defined population was given a positive chance of being selected. These probabilities were recorded and combined into an overall probability of selection for each UB participant and each nonparticipant, thus providing for the computation of unbiased estimates of population characteristics for UB as well as for comparison students. To produce these unbiased estimates, each student in each of the two samples was assigned a weight equal to the inverse of the probability of his being selected. Thus, students selected with smaller probabilities received larger weights. The weighting procedure used at the estimation stage therefore compensates for the use of unequal probabilities at the selection stage (see below, Section IV).

3. Special Base Year Subsampling Procedures for UB and Comparison Students Not Responding to the Fall (1974) Status Questionnaire

Mail returns of a Fall Status Questionnaire (FSQ), mailed as a fall 1974 follow-up instrument, fell far below expectations. The low response rate caused more than the usual amount of concern because it was felt that whether or not the sample member responded might well be highly related to whether or not the person was still enrolled in school. It was reasoned that a UB or comparison student who had dropped out of school would be more likely to have left home and less likely to have received and responded to the mailed FSQ. If such were the case, the school continuance rate among the nonrespondents could be far less than the continuance rate among respondents to the FSQ, and thus results based only on the approximately 50 percent who responded could well have been spurious.

Special steps were taken to obtain fall status information from those UB and comparison students not responding to the FSQ. A telephone-tracing procedure was employed wherein the sample UB or comparison student was telephoned, and the desired information was obtained from the sample individual by means of a telephone interview. For cases in which the individual himself could not be contacted, the desired information was sought from other knowledgeable individuals such as parents, project directors, school personnel, etc.

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[14] When a large proportion of one of the minority groups, i.e., Mexican American, Puerto Rican, or American Indian, was included in Stratum III, these students were placed in a separate stratum. The measure of size assigned this special stratum equaled the number of students in that stratum. The sample of seven students was then selected in proportion to the measures of size of the four strata. This extra stratification was done to increase the number of such minority students selected.

Although it would have been preferable to contact all FSQ nonrespondents by telephone, cost considerations made this prohibitive. Instead, a sampling plan was instituted. The most worrisome groups were those sample members from whom no response to any previous questionnaire had been obtained. All such individuals (exclusive of refusals)[15] were subselected with certainty into the telephone-tracing subsample. Of the remaining nonrespondents to the FSQ, all of whom had responded to a previous questionnaire, approximately 40 percent were randomly selected for inclusion in the telephone-tracing subsample. Compensation of sample weights for this subselection procedure was made prior to estimation.

III. SPECIFIC SAMPLING PROCEDURES FOR FOLLOW-UP STUDY

Since the basic sample of students for the followup study was the sample drawn during the base year, with certain ineligible sample members removed,[16] sampling during the follow-up study was confined to a special subsampling procedure for individuals who did not respond by mail to the Follow-up Questionnaire. The purpose and basic procedure for this subsampling was analogous to the similar subsampling in the base-year study (Subsection II.B.3 of this Appendix). Specifically, mail return rates had not been expected to exceed 50 percent, and subsampling of the group of nonrespondents, who were expected to differ from respondents, was planned to obtain survey coverage of the nonrespondent group through direct telephone contact. The number of nonrespondents to be selected was contractually set at 2,500, which at the time of contract negotiations was expected to represent over half of the nonrespondent group.

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[15] Any individuals who at any stage had refused to cooperate in the study were considered refusals for the remainder of the study, and were not contacted again.

[16] Sixty-four individuals of the original sample of 6,050 were declared ineligible for the survey due to various reasons (see Appendixes C and E)--41 of these ineligibles were former UB participants and 23 were from the comparison group.

The cutoff date for mail returns was set for 10 March 1977, at which time returns had been received from 1,862 individuals[17] of the 5,986 who were eligible for the survey. Of the remaining 4,124 individuals, some questionnaires had been returned by the U.S. Mail Services as "undeliverable" (N=953) and others had not been returned (N=3,171). RTI had anticipated these two categories of individuals (undeliverables and nonrespondents) and had planned to treat the categories as separate strata for purposes of subsampling. Moreover, there were several other possible strata for consideration in the subsampling. Specifically of concern were the strata of the original sampling (participant or nonparticipant, original grade cohort, and UB project association), certain background factors (race, sex, poverty status, and academic risk status), and availability of previous data (particularly data from the fall 1974 collection effort). The last category was considered important in that some planned analyses were to be conditional on an individual's status in fall 1974, and collection of current data from individuals for whom no fall 1974 data were available would not facilitate those analyses.

Toward resolving the issue of stratification, a meeting was held at RTI on 1 March 1974. In attendance were RTI study staff and USOE representatives from the Office of Planning, Budgeting, and Evaluation (OPBE) and the Division of Student Services and Veteran Programs (DSSVP). Decisions made during this meeting were based on mail returns and classifications as to "undeliverable" or "nonrespondent" status as of 24 February (at which time questionnaires had been returned by only 1,103 individuals). RTI analyses of response patterns indicated very little differential response of individuals in terms of original sampling categories or student background factors (see above), and, on the basis of these results, it was decided that these factors would not be considered in formal stratification. The formal stratifying factors were therefore reduced to nonresponse status (undeliverable or nonrespondent) and availability of fall 1974 data. Nonrespondents and those with available fall 1974 data were to be oversampled. The reasons for such oversampling were cost related; undeliverables were seen as more difficult to trace (and thus more costly), and it was seen as cost inefficient to collect current data from those without fall 1974 data.

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[17] Two of these returns had indicated that the sample member was deceased.

The number of individuals classified into each of the four major strata, the number selected from each stratum, and the selection rate for each stratum at time of sampling are given in Table A.3. The sampling rates are quite disparate among strata reflecting decisions of over-selection, and these disproportionate selection probabilities serve to decrease precision or increase the standard errors of estimates from resulting data (see below, Section IV of this Appendix). On the other hand, given a fixed budget for contacting the selected individuals and the relative ease with which nonrespondents could be contacted, more extensive efforts could be expended in attempts to contact those undeliverables selected. This was expected to result in a greater rate of contact and a subsequent reduction in standard errors. Under certain between- and within-strata variance assumptions and assuming contact rates of 70 percent and 40 percent for the nonrespondents and undeliverables, respectively, it was estimated that standard errors would be increased by a factor of 1.31. Since the contact rates actually obtained

Table A.3

STRATA SIZES, CASES SELECTED, AND SAMPLING RATES FOR FOLLOWUP STUDY SUBSAMPLING OF INDIVIDUALS NOT RETURNING QUESTIONNAIRE BY MAIL

Strata	Total Number of Cases	Number of Cases Selected	Sampling Rate
(1) Nonrespondents with available fall 1974 data	2,415	2,100	87.0%
(2) Nonrespondents without available fall 1974 data	756	250	33.1%
(3) Undeliverables with available fall 1974 data	771	130	16.9%
(4) Undeliverables without available fall 1974 data	182	20	11.0%

considerably exceeded the assumed rates (rates for strata 1 through 4 were 93.7 percent, 96.4 percent, 56.9 percent, and 75.0 percent, respectively), the figure given may be taken as an upper bound on the decrease in precision as a result of disproportionate subsampling.

Although the four strata previously defined were the only formal strata used in this subsampling, the other factors discussed above (e.g., base-year sampling factors and student background variables) were taken into account indirectly. The procedure used for sampling was a form of systematic random selection, in which the additional factors serve, in effect, as quasi strata. Specifically, within each of the four major strata, individuals were ordered on the basis of (1) former UB participation, (2) initial grade cohort, (3) an index reflecting sex, race, poverty status, and academic risk status, and (4) original project association. Within these ordered lists, selection was accomplished by: determining an interval, i, where i = total number of nonrespondents in the stratum divided by the number to be selected within strata; randomly selecting a number, k, between 1 and i; and selecting individual k and each ith individual following k on the ordered list. In this procedure, the ordering of the list stochastically ensures representation within the quasi strata.

IV. ESTIMATION

A. Estimation of Population Characteristics

The samples for this study were probability samples, and it is thus possible to make unbiased estimates of population characteristics from properly weighted sample data. The raw weights used in making these estimates are determined from the probabilities of selection. Each element in the sample was assigned a weight equal to the inverse of the probabilty of its being selected in the sample. All population estimates made from the sample data used these sample weights or some adjustment of these weights (see particularly Appendix E as related to balancing).

To obtain individual weights, the overall probability of selection for each student j, P_j, was first determined. For comparison students, the probability of selection is given by

$$P_j = \left(P_j^p\right)\left(P_j^s\right)\left(P_j^c\right)\left(P_j^e\right) ,$$

where

P_j^p is the probability of selecting the UB project associated with the school attended by individual j,

P_j^s is the probability of selecting the school attended by individual j, given that the associated project had been selected,

P_j^c is the probability of selecting the classroom containing individual j, given that the school had been selected, and

P_j^e is the probability of selecting individual j, given that his class had been selected.

Since UB students were selected with certainty, given selection of their project, the probability of selection of UB student j was

$$P_j = P_j^p .$$

The sampling weight, W_j, assigned to a student was then derived as the inverse of his overall probability of being selected;

$$W_j = \frac{1}{P_j} .$$

If responses were available for every sample individual in a specified subgroup, then an estimated total could be obtained by merely summing the weights of the sample individuals in that subgroup. An estimate of the number of female UB participants in September 1973, for example, could be obtained by summing the weights of all eligible female UB participants that had been selected into the sample. In general, let

X = a population total (e.g., the number of female participants in September 1973),

X' = an estimate of X, then

$$X' = \sum_{h=1}^{27} \sum_{i=1}^{2} \sum_{j=1}^{m_{hi}} W_{hij} X_{hij} ,$$

where

$$X_{hij} = \begin{cases} 1 & \text{if the jth student in the ith sample project in stratum h is female,} \\ 0 & \text{otherwise,} \end{cases}$$

W_{hij} = the sample weight of student j in sample project i of stratum h,

m_{hi} = the number of UB students in sample project i in stratum h,

and where this sum is taken over UB students only. Note also that a population proportion $P = \frac{X}{Y}$, where X is a subgroup of Y, can be estimated by $P' = \frac{X'}{Y'}$. Thus, to estimate the proportion of UB students who are female, X' would be computed as above and Y' would be computed as follows:

$$Y' = \sum_{h=1}^{27} \sum_{i=1}^{2} \sum_{j=1}^{m_{hi}} W_{hij} ,$$

where W_{hij} and m_{hi} are as defined above, and where the sum is again taken within the UB participant group. Proportions within the comparison group or within subsets of either the UB or comparison group would be derived in an analogous manner. Mean scores can be computed using the same procedure. In such cases X_{hij} is not merely an indicator variable taking on the values of 0 or 1 but instead is a discrete or continuous variable reflecting a measurement or score.[18]

The estimation procedures outlined above assume that information is available for the entire sample, which is rarely realized in survey research. Typically, a certain proportion of sample individuals fail to respond, for one reason or another. Nonresponse adjustment weights were computed for each student in the sample and were combined with the student weight to obtain the final student weight for a given analysis; following this weight adjustment, strictly analogous procedures of estimation were employed. Weight adjustments are discussed in greater detail in Appendix E.

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[18] Since a proportion is simply the mean of an appropriately coded dichotomous variable, computation of proportions is subsumed under the more general case.

B. Estimating Precision of Population Estimates[19]

Once the data were collected, it was possible to compute estimates of sampling errors from the sample data. The formulas used in computing estimates of sampling errors, although general in nature, will be presented in terms of an example, as an aid to the reader.[20]

Assume that one wishes to estimate both the number and proportion of UB participants who were enrolled in the eleventh grade in September 1973 and who enrolled in the twelfth grade in September 1974, and to estimate the sampling errors of the estimated number and estimated proportion. Let

X = population count of UB participants enrolled in eleventh grade in September 1973 who subsequently enrolled in the twelfth grade in September 1974,

Y = population count of UB participants enrolled in the eleventh grade in September 1973,

X' = estimate of X,

Y' = estimate of Y.

For each of the 27 strata, indexed by h, one can define:

X_h = population number of UB participants in UB projects classified as in stratum h, who were enrolled in the eleventh grade in September 1973 and who had enrolled in the twelfth grade in September 1974,

Y_h = population number of UB participants in UB projects classified as in stratum h, who were enrolled in the eleventh grade in September 1973.

Let X'_h estimate X_h and Y'_h estimate Y_h .

Now, $\sum_{h=1}^{27} X'_h = X'$ and $\sum_{h=1}^{27} Y'_h = Y'$.

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[19] For specific procedures of estimating precision in the case of balanced weights, see Appendix E.

[20] See Chapter 6 of Kish, L., (1965), Survey sampling, New York: John Wiley & Sons, for more detailed development of estimation procedures.

Let A and B be the two sample projects in a stratum, then

$$X'_h = X'_{hA} + X'_{hB} ,$$

and

$$Y'_h = Y'_{hA} + Y'_{hB} ,$$

where

$$X'_{hi} = \sum_{j=1}^{m_{hi}} W_{hij} X_{hij} , \quad i = A \text{ or } B,$$

$$Y'_{hi} = \sum_{j=1}^{m_{hi}} W_{hij} Y_{hij} , \quad i = A \text{ or } B,$$

and where

W_{hij} = the weight for UB student hij,

m_{hi} = the number of sample UB students in project hi,

$$X_{hij} = \begin{cases} 1 & \text{if UB student hij was enrolled in the eleventh grade in September 1973 and in the twelfth grade in September 1974,} \\ 0 & \text{otherwise,} \end{cases}$$

$$Y_{hij} = \begin{cases} 1 & \text{if UB student hij was enrolled in the eleventh grade in September 1973,} \\ 0 & \text{otherwise.} \end{cases}$$

The desired estimate X', as shown above, is obtained from the sample as

$$X' = \sum_{h=1}^{27} X'_h = \sum_{h=1}^{27} (X'_{hA} + X'_{hB}) .$$

The estimated error variance for X' would be computed as

$$VAR(X') = \sum_{h=1}^{27} (X'_{hA} - X'_{hB})^2 ,$$

and the estimated standard error would be obtained as

$$SE(X') = \sqrt{VAR(X')} .$$

It should be noted that the variance estimator given above is unbiased for with-replacement selection of projects, but provides a slight over-estimate of the total variance for without-replacement sampling of projects, due to ignoring the finite population correction at the project selection stage. The estimator does, however, properly account for within-project sampling error and for measurement error. In addition, the form of the estimator is simple and easy to compute compared to the complex multistage formulas required for unbiased estimates of sampling variance for selections without replacement. These so-called unbiased formulas provide an underrepresentation of measurement error.

The population proportion one wishes to estimate is $P = \frac{X}{Y}$, which could be estimated from the sample by forming $P' = \frac{X'}{Y'}$. The error variance for this ratio estimate, P', would be computed as follows:

$$VAR(P') = [VAR(X') - 2(P')COV(X',Y') + (P')^2 \, VAR(Y')]/(Y')^2 ,$$

where

$$VAR(X') = \sum_{h=1}^{27} (X'_{hA} - X'_{hB})^2 ,$$

$$VAR(Y') = \sum_{h=1}^{27} (Y'_{hA} - Y'_{hB})^2 ,$$

$$COV(X',Y') = \sum_{h=1}^{27} (X'_{hA} - X'_{hB})(Y'_{hA} - Y'_{hB}).$$

The standard error of P' is

$$SE(P') = \sqrt{VAR(P')} .$$

The given variance approximation provides a slight underestimate of the error variance of a ratio.

There is a more compact way of expressing the formula for estimating the error variance of the ratio $P' = \frac{X'}{Y'}$. Let

$$Z'_{hi} = \frac{1}{Y'} (X'_{hi} - P' \, Y'_{hi}), \quad i = A \text{ or } B.$$

Then,

$$\mathrm{VAR}(P') = \sum_{h=1}^{27} (Z'_{hA} - Z'_{hB})^2.$$

The standard error would, of course, still be obtained by taking the square root of the error variance.

In a similar fashion, the error variance for the estimated difference between two ratios can be obtained. To obtain the error variance of $\Delta' = P'_1 - P'_2$, where P'_1 is defined as P' above and $P'_2 = \frac{U'}{V'}$, where U' and V' are analogous to X' and Y', respectively. Form Z'_{hA} and Z'_{hB} as above, and similarly compute

$$T'_{hi} = \frac{1}{V'} (U'_{hi} - P'_2 V'_{hi}),\ i = A \text{ or } B.$$

Then define

$$D'_{hi} = Z'_{hi} - T'_{hi}\ ,\ i = A \text{ or } B.$$

The error variance can then be determined by[21]

$$\mathrm{VAR}(\Delta') = \sum_{h=1}^{27} (D'_{hA} - D'_{hB})^2\ ,$$

and the standard error is obtained as

$$\mathrm{SE}(\Delta') = \sqrt{\mathrm{VAR}(\Delta')}\ .$$

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[21] Woodruff, R. S., Simple Method for Approximating Variance of a Complicated Estimate. Journal of the American Statistical Association, June 1971, Vol. 66, Number 334, pp. 411-414.

Appendix B

Follow-Up Questionnaire and Survey Materials

Appendix B

Follow-Up Questionnaire and Survey Materials

This appendix contains the UB Follow-Up Questionnaire, letters, and other materials that were used in the data collection effort for the UB follow-up evaluation. Section I contains the materials sent to study participants in the first mailout. The first mailout package consisted of an introductory letter from the Assistant Commissioner of Education for Planning and Evaluation; an explanation of and directions for the follow-up questionnaire; and the follow-up questionnaire postcard.

A second package was mailed to study participants who did not respond to the first mailout. This second package consisted of a reminder letter from the RTI Project Director and the complete package used in the first mailout (including the introductory letter, explanation and directions, and follow-up questionnaire postcard). The reminder letter is provided as Section II.

I. First Mailout Questionnaire and Materials

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
OFFICE OF EDUCATION
WASHINGTON, D.C. 20202

January 28, 1977

Hello,

It has been three years since we first contacted you as a participant in a nationwide study of young people. At that time, you were asked to take part in a national survey to be conducted by the Research Triangle Institute and the U. S. Office of Education. The purpose of this study, you will recall, is to find out about the needs of young adults who may use the services of several educational and occupational programs sponsored by the Federal government.

Our last contact with you was two years ago, in the fall of 1974, when we sent you a brief questionnaire to fill out. Your responses to the previous survey, along with those of the other study participants, were very helpful. Some of the preliminary results have already been presented to the Congress of the United States for their deliberations. Again, we would like for you to tell us how you are progressing toward goals you set for yourself two or three years ago.

Enclosed you will find a questionnaire that we are sending to all of the original group who were selected for the study. The completion of this short postcard questionnaire should take no more than two or three minutes of your time. We have made every effort to make all questions clear and precise and to make certain that all questions are relevant. You can answer most of the questions by simply circling the number corresponding to the choice that describes what you are doing.

By completing this questionnaire you can help your Federal government make wise decisions. Your responses are valuable information. It is very important that you complete and return the postcard. There were only a few of you selected for the study from among the millions of high school students in 1973. Although all of the originally selected group are being asked to provide this information, we need answers from all of you to have a true picture of what young adults as a group are doing and we want to be sure that each of you has the opportunity to be represented in the final results. Please answer all the questions; the more complete your reply, the more helpful it will be in this important research. There is a place for a response to every question. After you have answered these few questions, just fold the postcard over, seal it so that your answers will not be revealed, and drop it in a mailbox.

We are enclosing a separate information sheet which tells you how your responses are protected and provides instructions for completing the questionnaire. We want to emphasize that your answers will never be identified with you. Your name, as it is related to your answers, will be known only to a small number of professional researchers involved with this study.

Thank you for your cooperation in this important study.

Sincerely,

John W. Evans

John W. Evans
Assistant Commissioner
Office of Planning, Budgeting
and Evaluation

Enclosure

HOW YOUR RESPONSES ARE PROCESSED

Many people are concerned about the matter of privacy, which is an area of major concern for all our project staff members. When questionnaires are received at the Research Triangle Institute, each person is assigned a number. Only numbers are used from that point on in handling the data. The number has no relation to any other number previously assigned to you, such as a driver's license or Social Security number. Any corrections to your name and address are entered in a special secured file for that purpose and is never entered in the data file.

Once the number has been assigned, a survey specialist checks your responses to make sure that we understand all your answers. Your name and address are then deleted from the questionnaire and the answers are given to a machine operator, who enters your responses, along with the code number, directly into a protected computer file which can only be examined by a few project staff members who know the computer keyword necessary to have access to the file.

All individual questionnaire responses are therefore protected, and the questionnaires themselves are stored in a secure area until the study is completed, at which time they are destroyed.

DIRECTIONS

The postcard questionnaire consists of eight questions plus a space to correct your name and address if we had them wrong and a space to write in your telephone number. Please read carefully and answer each question. It is important that you follow the directions for responding, which are

- (Circle only one)
- (Circle all that apply).

In one case you are asked to fill in the name and location of your current (or last attended) school.

(OVER)

When you are asked to circle a number, please make a heavy circle. Circle as many numbers as the directions indicate for each question. Here are examples, one for each type of question.

Are you currently working?
(Circle only one)
[✓ 1]—No
2—Yes, full-time
3—Yes, part-time

If you are not currently working, then you would answer this question by circling the number corresponding to "No" as shown above.

Did you ever participate in an Upward Bound program?
(Circle all that apply)
1—Never
2—Yes in grade 10 or before
[✓ 3]—Yes in grade 11
[✓ 4]—Yes in grade 12

If you participated in an Upward Bound program in both the 11th and 12th grades, then you would answer this question by circling both 3 and 4 (but not 1 or 2) as shown above.

When you have completed the questionnaire, make any corrections to your name and address in the space provided, fill in the telephone number where you can be reached, fold the postcard over, seal it, and mail it back to us. No postage is necessary.

This study is authorized by law (20USC 1226). While you are not required to respond, your cooperation is needed to make the results of this survey comprehensive, accurate, and timely.

From ______________________

____

____

____

FIRST CLASS
Permit No. 593

Business Reply Mail
No Postage Stamp Necessary If Mailed
in the United States

First Class

— Postage will be paid by —

Research Triangle Institute
Post Office Box 12194
Research Triangle Park, North Carolina 27709

Project 22U-889-20
Confidential Material—
to be opened only by
authorized project personnel

Follow-Up Questionnaire for Nationwide Study of Young Adults

My name and address are

___ Both OK as printed

___ Correct my name to ______________________________
First Middle Last

___ Correct my address to ______________________________
Street, Route, and/or Box Address

____
City Street Zip Code

The telephone number at which I can be reached is ____________________
Area Code Number

1. What is the highest educational level you have completed? *(Circle only one)*	High School	Trade or Vocational School	College University
	1-- 10th grade	4-- Less than 1 year	7-- 1st year
	2-- 11th grade	5-- One year	8-- 2nd year
	3-- 12th grade	6-- Two or more years	9-- 3rd year
			10-- 4th year

2. Are you currently working? *(Circle only one)*	1-- No 2-- Yes, full-time 3-- Yes, part-time
3. Are you currently serving in the military? *(Circle only one)*	1-- I've never been in the service 2-- I'm in the service now 3-- I'm now a veteran
4. Are you currently enrolled in any school? *(Circle only one)*	1-- Not enrolled 2-- Enrolled part-time 3-- Enrolled full-time

5. Please give the exact name and location of the school you are attending or last attended. *(Please do not abbreviate)*

School Name ______________________________

City ____________________ State ____________________

6. What is the greatest amount of schooling you expect to complete? *(Circle only one)*	1-- High school only 2-- Vocational, trade, or business school 3-- Some college 4-- Finish 4 year college (BA, BS) 5-- Graduate or professional degree (MA, PhD, MD)
7. What problems have you had in getting the kind of education you would like to get? *(Circle all that apply)*	1-- Poor grades 2-- Poor high school preparation 3-- Lack of personal or family finances 4-- Inadequate financial aid 5-- Program I wanted is not available 6-- Family responsibilities
8. Did you ever participate in an Upward Bound Program? *(Circle all that apply)*	1-- Never 2-- Yes in grade 10 3-- Yes in grade 11 4-- Yes in grade 12

THANK YOU FOR YOUR COOPERATION

OE Form 524

O.M.B. No. 51-S76047
Approval Expires June 30, 1977

II. Second Mailout Materials

RESEARCH TRIANGLE INSTITUTE
POST OFFICE BOX 12194
RESEARCH TRIANGLE PARK, NORTH CAROLINA 27709

CENTER FOR EDUCATIONAL RESEARCH AND EVALUATION

February, 1977

Dear Study Member,

About two weeks ago we mailed you a letter from the Assistant Commissioner of Education of the United States, which requested your continued participation in a national study of young adults. We have not yet heard from you, and we are therefore mailing you this reminder in case you may not have gotten around to sending us the requested information. In case you have misplaced the original package, we are including duplicates of the materials originally mailed to you.

I would like to take this opportunity to stress that your response is very important and to urge you to complete and return the very short enclosed questionnaire, if you have not already done so. This will require very little of your time and will assist us in providing your government with valuable information.

Thank you for your help in assisting us with this important research.

Very truly yours,

G. J. Burkheimer, Jr.
Project Director

GJBJR/sb

Enclosure

(919) 549-8311 FROM RALEIGH, DURHAM AND CHAPEL HILL

Appendix C

Data Collection and Preliminary Processing Procedures

Appendix C

Data Collection and Preliminary Processing Procedures

I. OVERVIEW

This appendix describes the procedures followed in data collection for the first follow-up of the national evaluation of the Upward Bound (UB) program. Instrument development, mail and telephone survey activities, obtaining data from other available data bases, and preliminary receipt control and manual processing procedures are the subjects of this appendix. Other activities related to data collection are detailed in other appendixes: computerized receipt control and processing of collected data, in Appendix D; instruments and survey material, in Appendix B; and subsampling for telephone follow-up of mail nonrespondents, in Appendix A.

Data collection activities of the follow-up study consisted of a mail survey, using a very brief postcard instrument, of 6,050 former UB participants and nonparticipants who had been selected for the base-year study. The mail survey consisted of an initial mailing followed by a mailed reminder to those who had not responded to the initial mailing. A sample of nonrespondents to the mail survey were selected for telephone interview to gather the information requested on the postcard instrument. Additional data for the study were obtained from existing sources. Student background information, prior school status, and prior UB participation status were available from the master file developed during the base-year study. Information regarding the postsecondary education (PSE) institutions attended by sample members was obtained from an institutional data file constructed by the College Entrance Examination Board (CEEB) and from records maintained by the Division of Student Services and Veterans Programs (DSSVP) of USOE.

To facilitate data collection and processing operations, the entire survey was monitored by a computerized control system. Mailed returns were manually edited for completeness and consistency and subsequently coded; returned instruments that "failed edit" initiated telephone contact to clarify or complete the questionnaire responses. Data were then coded into machine readable form and merged into a master data file through machine edit programs and the survey control system. The general flow of data collection and preliminary processing activities of the study is given in Figure C.1.

RTI maintained concern for aspects of the study related to anonymity of respondents and confidentiality of their responses, and took every reasonable step to insure that the requirements of the law in this regard were met in both spirit and letter. For the follow-up study, the Institute's Committee for the Protection of Human Subjects was involved in all relevant aspects of this survey for the protection of USOE, RTI, and the study respondents.

II. INSTRUMENT DEVELOPMENT

The data collection instrument used in the follow-up study was intended to collect only the information pertinent to specific data needs of USOE with a minimum burden on the respondent in completing and returning the questionnaire. The medium for collecting the information was a postcard questionnaire. By using a postcard, responses could be entered, the card folded, sealed (for confidentiality purposes), and placed directly into the mail service. Major concerns in preparation of the instrument were: (a) to collect data that would address the study analysis questions (see Chapter 1), (b) to facilitate response to the instrument, and (c) to maintain clarity and readability of the questionnaire items (to minimize response errors and keying errors).

In addition to items related to the basic analysis questions, the questionnaire called for corrections to the individual's name and address and for a current telephone number. There were two basic reasons for requesting this information. One reason was to increase the face validity of the data collection effort; providing the respondent with an opportunity to correct the name and address was presumed to give him a sense of concern for his individuality rather than seeing himself only as a number. The other reason was that a procedure for telephone follow-up was planned to collect critical data items that failed edit on returned postcards; name and address corrections and telephone numbers were quite helpful in these efforts.

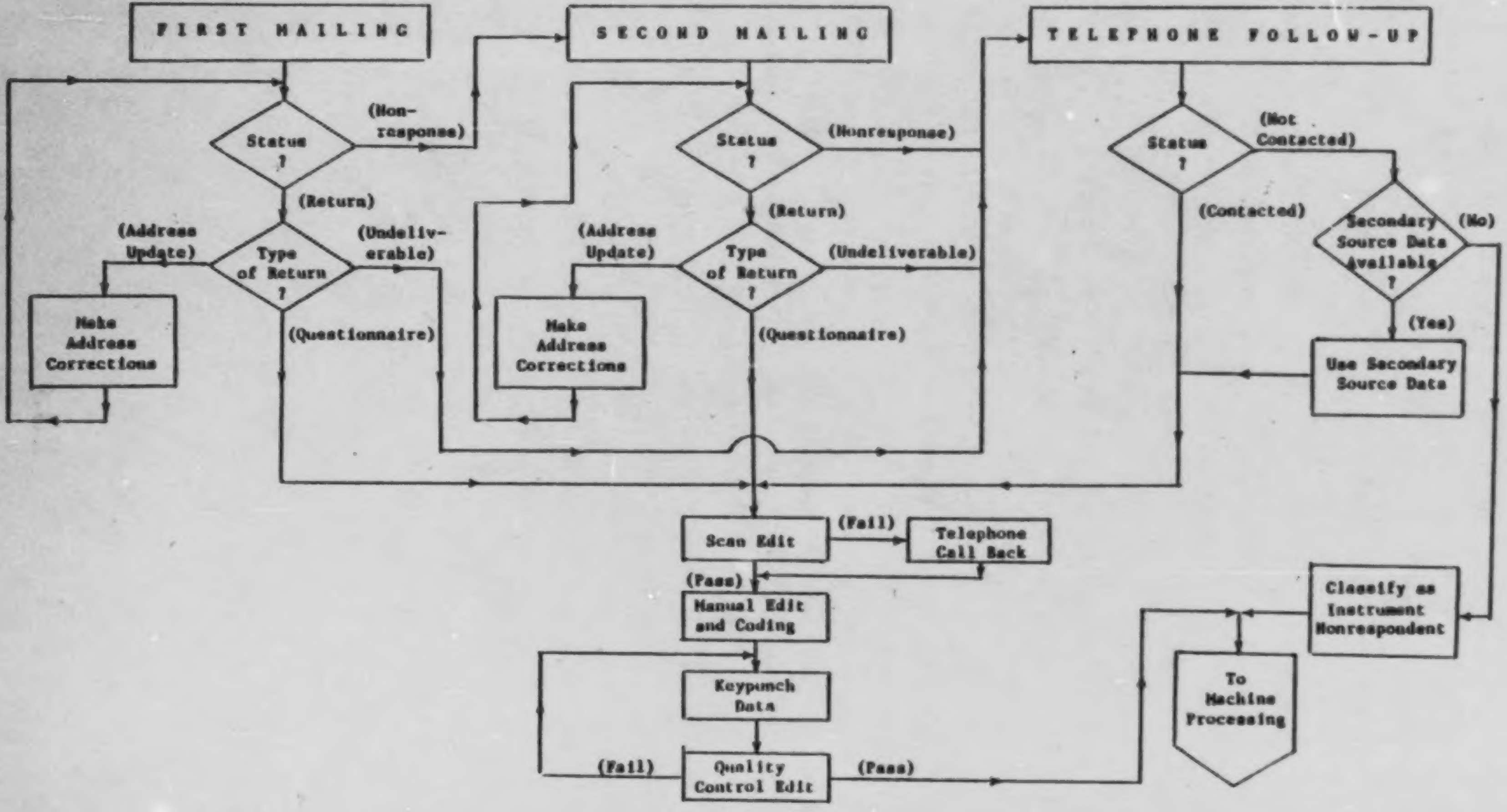
FIRST MAILING
SECOND MAILING
TELEPHONE FOLLOW-UP
Status ?
(Non-response)
(Return)
(Address Update)
Type of Return ?
(Undeliverable)
Make Address Corrections
(Questionnaire)
Status ?
(Nonresponse)
(Return)
(Address Update)
Type of Return ?
(Undeliverable)
Make Address Corrections
(Questionnaire)
Status ?
(Not Contacted)
(Contacted)
Secondary Source Data Available ?
(No)
(Yes)
Use Secondary Source Data
Scan Edit
(Fail)
Telephone Call Back
(Pass)
Manual Edit and Coding
Keypunch Data
(Fail)
Quality Control Edit
(Pass)
Classify as Instrument Nonrespondent
To Machine Processing

Figure C.1. General Flow of Data Collection and Preliminary Processing Activities for the Upward Bound Follow-Up Study.

The questionnaire also provided space for the inclusion of a duplicate mailing label, containing the individual's name and address (as currently indicated in RTI files) and a unique 5-digit identification (ID) number (including a check digit).[1] There were two reasons for including the duplicate mailing label. First, inclusion of the label on the questionnaire portion of the postcard provided immediate reference for the individual in correcting his name and address. Second, the presence of this label simplified the processing of returns (which were to be added to data from the base-year study through a matching with the unique ID code).

A cover letter explaining the purpose of the study and soliciting cooperation, as well as an information sheet providing directions for completing the questionnaire and an explanation of how privacy of data was maintained, were also prepared to accompany the instrument. A copy of the postcard instrument, accompanying letters, and the information sheet are provided in Appendix B.

The content and general format for the Upward Bound follow-up instrument was provided by OPBE on 1 October 1976. On 8 October 1976, the instrument package with three alternative forms of the questionnaire was forwarded to OPBE for consideration. On 15 October, OPBE provided suggested changes in the format of the instrument and in the accompanying material. These suggestions were incorporated into the instrument package for inclusion in the Office of Management and Budget (OMB) support statement which was initially mailed to OPBE on 20 October 1976. OPBE suggestions for changes in the support statement were received on 26 October, and the final OMB package was mailed to OPBE on 28 October after incorporation of the suggested changes. OMB clearance of survey instruments was received on 14 January 1977.

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[1] This ID number was not the same ID number used for individuals in data files of the base-year and follow-up study, but could be matched with that ID number through a separate conversion file.

III. SURVEY OPERATIONS

The follow-up survey of the evaluation of Upward Bound was a continuation of a data collection effort to obtain information about the sample of 6,050 UB participants and nonparticipants who were selected for the base-year survey. The base-year study was initiated as a nationwide, multistage, stratified probability sample. The first stage involved selecting UB projects located in the coterminous United States and serving students in the tenth through twelfth grades in September or October 1973. All tenth-, eleventh-, and twelfth-grade UB participants in the selected projects were included in the survey. The comparison group was selected by associating schools with each UB project, selecting a probability sample of such schools, selecting a sample of homeroom classes within the sample schools, and finally selecting students within classes (see Appendix A for details of base-year sampling).

The follow-up survey involved sending a self-administered postcard questionnaire to the sample members. Since the nonresponding population was expected to differ from the responding population in terms of high school and postsecondary educational success, a probability sample of 2,500 mail nonrespondents was selected for telephone follow-up (see Appendix A). The steps involved in survey operations are generally defined in Figure C.1 above; the details and results of these data collection activities are presented in this section.

A. First Mailing

Of the original data base of 6,050 students selected for the base-year study, 64 cases were considered ineligible for the follow-up study, leaving 5,986 students eligible for the mailing. The 64 students considered ineligible consisted of: (1) 21 comparison students from a single project for whom names had never been established during the base-year study due to high school refusal; (2) two UB participants from a single project, whose names had never been established in the base-year study, due to an operational oversight during field operations; (3) 40 students who had been identified as either not in high school or at a grade level less than 10 at time of sampling for the base-year study;

and (4) one student who had been represented by duplicate records in the base-year file (both as a UB participant and as a comparison student--the comparison record was removed). Those cases in categories (3) and (4) above were not defined within the sampling frame for the base-year study, and their exclusion from the follow-up study was completely warranted. Those in categories (1) and (2) should have been available for follow-up but clearly could not be reached; the sampling weights associated with these cases were therefore appropriately redistributed to other students in the sample.[2]

Mailing labels, including a unique ID number and a code indicating first mailing, were generated by computer from the name and address file previously established by RTI during the base-year study. This file was maintained in a password-protected computer file and contained last known name, address, telephone number, and encrypted ID number for each member of the base-year study sample. Two labels were generated for each individual, one for the envelope in which the instrument package was mailed and one to be included on the questionnaire (to facilitate processing of returns). Of the 5,986 cases eligible for the first mailing, 339 did not have addresses entered in the name and address file,[3] and, therefore, questionnaires were mailed to only 5,647 sample members. The actual mailing of questionnaires began on 27 January 1977 and was completed on 31 January 1977. The packet mailed included the postcard questionnaire, a letter from the Assistant Commissioner of Education, and an information sheet (see Appendix B).

Returns from the first mailout of questionnaires began to arrive at RTI in early February. These returns comprised three separate categories: (1) questionnaire responses, (2) questionnaires returned as undeliverable, and (3) Mail-Service-supplied address changes. The final count of returned questionnaires to the first mailing was 1,461, or 25.9 percent of those mailed. A more complete and detailed picture of survey returns is presented in Subsection II.D below.

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[2] For those in category (1), this had already been accomplished during the base-year study. For the two cases in category (2), the weight distribution was to other students within the same project, grade level, and balancing category (see Appendix E for weight adjustment techniques).

[3] Examination of the previous response histories of the 339 cases and subsequent resort to hard copy from the base-year study indicated that addresses were available for some of the students but somehow had not been entered into the name and address file. Addresses for others had never been obtained (specifically those for whom only secondary source data had been collected in the fall 1974 telephone survey of previous nonrespondents).

B. Second Mailing

The second mailing took place on 17 and 18 February 1977. The cutoff date for first mailing return (to define eligibility for second mailing) was established as 15 February 1977. Of the 5,647 questionnaires sent out in the first mailing, 1,003 had been returned by sample members, 671 had been returned as undeliverable, and one had been returned with notation indicating that the sample member was deceased, at time of cutoff. Second mailings were sent to the 3,972 cases remaining; address corrections had been processed at least once for 144 of these cases. Additionally, addresses had been found for 233 of the 339 individuals for whom no addresses were available at time of first mailout. These 233 cases were included in the second mailout, for a total mailing to 4,205 individuals.

Mailing labels were generated by computer from the current corrected name and address file as in the first mailing except that a code indicating second mailing was produced. The packet included in the second mailing contained the material for the first mailing and a prompting letter from the RTI project director (see Appendix B). The final count of returned questionnaires to the second mailing was 788 or 18.5 percent of those mailed; however, the pattern of mail returns suggested that the second mailing prompted many other individuals to return the questionnaire sent them in the first mailing.

C. Telephone Follow-Up Survey

Plans for the study had included a telephone follow-up survey of those individuals not responding to the mail questionnaire, since it had been anticipated that mail respondents would differ from nonrespondents in terms of the outcome measures being assessed by the study. Since cost considerations precluded contacting all nonrespondents, a stratified subsample of 2,500 individuals were selected for telephone follow-up on the basis of mail return status as of 11 March 1977. On that date, questionnaires had been returned by mail from 1,862 individuals, so that the overall selection rate for telephone follow-up was 60.6 percent of the 4,124 nonrespondents at that time.[4]

Sampling was conducted on 11 March 1977, at which time labels were computer generated for the telephone interview forms (i.e., copies of the mail questionnaire). Labels were affixed to the telephone interview forms, and the forms were delivered to the telephone interviewers on 14 March. Some of the individuals sampled for telephone follow-up represented those cases for whom mailed material had been returned as "undeliverable;" others represented the 106 cases for whom no address could be found in the base-year hard copy.[5] These two categories of individuals comprised a group that was considerably more difficult to locate than those who had simply failed to respond to the questionnaire mailing. RTI received considerable assistance from USOE in attempting to locate these individuals. Staff of DSSVP initiated a procedure whereby UB project directors were contacted by personal telephone call in an effort to obtain current information about the location of the individuals of concern. DSSVP efforts were quite successful, leading to a considerably greater contact rate for this group than otherwise would have been realized.

The telephone tracing and interview procedures continued through 6 May 1977 and involved an attempt to reach mail nonrespondents and undeliverables to collect, by telephone, information that had been requested by mail previously. During the location process, questionnaire information about the sample member was solicited from family members and other secondary sources. Any information so collected was used if the sample member could not be personally contacted; however, obtaining these secondary source data in no way restricted further attempts to contact the individual. Telephone interviewers used two forms on which to collect the information and a manual developed to assist them. The two forms were copies of the mail instrument and differed only in color (for ease in subsequent differentiation between primary and secondary source data).

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[4] Selection rates were quite different within the various sampling strata employed. For details of stratification and sampling of individuals for telephone follow-up, see Appendix A, Section III.

[5] Questionnaires had never been mailed to those cases for whom address information had not been found in hard copy searches; however, by sampling this group for telephone survey, group members had positive probability for representation.

To eliminate unnecessary expense, mail returns were checked against current lists of telephone interview completions; and, if a mail return was received for a sampled individual prior to completing telephone interview procedures, subsequent telephone activities for that person were suspended. Some mail returns were not received in time to avoid duplication of data collection efforts, but a substantial number was received prior to telephone interview. Of the 2,500 individuals selected for telephone interview, information was collected (by telephone from primary or secondary source or by mail return) on 2,298, for a 91.9 percent overall contact rate.[6] The rate was lower (59.3 percent) among those individuals for whom RTI did not have current address information.

D. Total Survey Response Rates

At close of the business day on 6 May 1977, all data collection activities were terminated. Final return rates for the various data collection efforts (mail and telephone surveys) are given in Table C.1. As can be seen from the values presented, the total survey response rate was over 70 percent of those eligible.

Final data availability in relationship to the major study classifiers (UB participation and initial grade-level cohort) is given in Table C.2. The values presented in Table C.2 indicate some differential data availability. In general, data were collected from a larger proportion of the UB group than the comparison group, reflecting the greater ease with which they could be traced through their previous project director, but data availability seemed to be relatively independent of original grade level.

IV. PRELIMINARY PROCESSING OF DATA RETURNS

As survey returns were received at RTI, preliminary processing activities were implemented to facilitate computerized survey control

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[6] This figure includes ten of the original sample members determined to be deceased.

Table C.1

FINAL RESPONSE RATES FOR THE UPWARD BOUND FOLLOW-UP STUDY

Group	Number	Percent of Eligibles
Ineligibles[a]	77	--
Nonrespondents[b]	1,752	29.3%
Respondents		
Mail 1 (M1) only	1,365	22.9
Mail 2 (M2) only	669	11.2
Telephone (T) only	2,041	34.2
M1 and M2	65	1.1
M1 and T	27	0.5
M2 and T	50	0.8
M1, M2, and T	4	0.1
Total Respondents	4,221	70.7
Total Eligibles	5,973	100.0
Total	6,050	--

[a] Includes 13 deceased sample members.

[b] Includes "undeliverables"

Table C.2

DATA AVAILABILITY WITH RESPECT TO PRIOR UPWARD BOUND PARTICIPATION STATUS AND ORIGINAL GRADE COHORT

Fall 1973 Grade Level[a]	Upward Bound Participant Status[b]			
	Prior Participant		Nonparticipant	
	Number of Cases[c]	Data Available[d]	Number of Cases[c]	Data Available[d]
10	564	393 (69.7%)	767	510 (66.5%)
11	1,410	1,072 (76.0%)	770	505 (65.6%)
12	1,691	1,247 (73.7%)	771	494 (64.1%)
TOTAL	3,665	2,712 (74.0%)	2,308	1,509 (65.4%)

[a] Classification used is best estimate of original grade cohort.

[b] Classification used is that used for original sampling.

[c] The 77 cases who were ineligible for the survey or who were reported to be deceased are not included in these counts.

[d] Percentages expressed are relative to number of cases in the specific cell.

and to prepare data for subsequent computer processing and analysis. These activities, which are the subject of this section, can be categorized into two basic areas: (a) processing events and (b) processing returned data. Although the actual processing of events and returns took place from February through May 1977, tasks preliminary to these activities (e.g., specification of processing rules, development of editing and coding manuals, and training of editors and coders) took place prior to the first questionnaire mailing.

A. Event Processing

Although daily receipt logs and tabulations of returns were maintained, this survey was controlled primarily by a computerized survey support system that monitored the operational aspects of the follow-up study from the time data collection activities were initiated until an edited machine-readable data base was established. Monitored events included mailout of materials, execution of telephone interviews, return of questionnaires, manual and machine editing, and nonrespondent sample selection. In general, the system maintained data that reflected the current survey status of each sample member as well as useful historical information describing the individual's sequencing through the various event steps. In addition to monitoring the survey activities, the system could provide, on request, statistics on each of the operational categories determined by the events that had been defined (e.g., current response status). Although the details of the support system are documented elsewhere in this report (Appendix D), the specific events recorded and the procedures for preparing events for the system are presented here.

The specific events monitored in this survey for each of the individuals eligible for the follow-up study are shown in Table C.3. It should be noted that some of the events were computer generated, and thus did not require manual processing. As any of the events occurred for an individual, the event was manually coded, keypunched, and entered into the computerized survey control system on an event card containing the individual's unique ID number. For example, the first operation performed when a questionnaire was returned to RTI was to code an event card for the individual involved indicating the type of return (event 2,

Table C.3

MONITORED STUDY EVENTS

Event	Description
1[a]	First mailout of instrument (generated by computer at time of label generation).
2	First mailout returned by Mail Service as undeliverable.
3	First mailout returned by sample member.
4	Forwarding address correction returned by Mail Service (first or second mailout).
5[a]	Address correction made to name and address file (first or second mailout).
6[a]	Second mailout of instrument (generated by computer at time of label generation).
7	Second mailout returned by sample member.
8	Second mailout returned by Mail Service as undeliverable.
9	Returned questionnaire failed scan edit (first or second mailout).
10	Returned questionnaire that previously failed edit was successfully resolved (first or second mailout).
11[a]	Selected for telephone survey (generated by computer at time of sample selection).
12	Telephone interview successfully completed for all key questionnaire items (1, 4, 5, 8).
13	Telephone interview completed but not all key questionnaire items were collected.
14[a]	Data entered onto Master File (generated by computer at time of data entry).

[a] These events were generated by computer.

3, 7, or 8, as defined in Table C.3). Operations for processing events through to the survey control system were such that events of any type were reflected in the control system within a few days of their occurrence.

B. Processing Returned Data

The steps of preliminary processing of returned data are generally defined in Figure C.1 above. Other than the processing of address changes provided by the Post Office (which involved no more than coding and keypunching of the ID number and address change) these activities were directed toward ensuring quality data preparation from the returned questionnaires. Given the brevity of the instrument, the procedures were not complex.

The first step in processing was a scan edit of returned questionnaires. This procedure was completed as soon as possible after receipt (and event-coding of that receipt) and involved only Items 1, 4, 5, and 8 of the questionnaire, which were considered to be the key items of information. If any of these items were incomplete, indeterminate, or inconsistent,[7] then the questionnaire failed the scan edit and was sent to the RTI Telephone Tracing Department for resolution. Resolution consisted of a telephone call to the individual to obtain the incomplete information or to resolve the inconsistency.

Following scan edit (and resolution, if required), the next step in processing involved additional manual edit and coding. The major additional manual edit was a check for multiple or unclear responses to items 2, 3, and 6.[8] For items 2 and 3, multiple responses led to a coding of the item as indeterminate. For item 6, the numerically highest circled multiple response was coded as the individual's response.[9]

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[7] Indeterminancy for items 1 and 4 consisted of multiple responses or unclear circling of response options. Item 5 was indeterminate if the name of the PSE institution given by the respondent did not provide enough information to uniquely identify the campus of interest. For example, University of North Carolina, without an address, would have been considered indeterminate, since there are multiple campuses of UNC. Item 8 was indeterminate if an individual indicated both that he had participated in UB and that he had never participated in UB. Additionally, it was possible for the responses to items 1 and 5 to be inconsistent. Specifically, a response to item 1 indicating completion of some PSE was considered inconsistent with a response to item 5 that the last school attended was a high school.

[8] Multiple responses were appropriate for item 7, and thus no manual edit was performed for that item.

All coding for the follow-up study, including that performed during manual edit, was done directly on the hard copy, which had been designed for direct data transcription. Some coding was required for item 5 of the questionnaire, an open-ended item requesting name of school last attended. For each listed PSE institution, a five-digit Federal Interagency Committee on Education (FICE) code with a check digit was entered directly onto the questionnaire (a unique code was used for any high school listed in item 5). Since a mailing label, which included a unique ID number, had been attached to the questionnaires and to the telephone interview forms, it was not necessary to code the individual's ID number.

Following manual edit and coding, data were keypunched and verified directly from the coded questionnaire. Listings of the resultant data cards were subjected to a visual scan to detect obvious column misalignments and out-of-range punches. Additionally, a 10 percent sample of keypunched data was subjected to a quality control edit wherein listings of punched cards were compared directly to hard copy. These procedures indicated extremely low keypunch error rates. Those few errors that were detected were returned for repunching and verification. On completion of keypunching and edit, the cards were delivered for computer edit procedures and subsequent file construction, as detailed in Appendix D.

C. Confidentiality of Project Data

RTI assumed the responsibility for maintaining all project documents--including questionnaires and data tapes--under strict control to insure not only confidentiality but also the integrity of the various data. During all stages of processing and storage, senior project personnel controlled access to and removal and replacement of all documents from specified working and storage areas.

Follow-up materials were received in the mail on a daily flow basis and, insofar as possible, were dealt with on a flow basis. Overnight storage of processing batches was in a locked and secure work space. When processing was completed, source documents were filed in an ordered, accessible manner in a controlled-access storage space.

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[9] Due to the ordered nature of the alternatives provided for item 6, this editing rule was considered logical and appropriate.

The name and address file that was necessary for distribution purposes and the substantive data files required for analysis purposes were maintained separately on a strict need-to-know basis throughout the duration of the project. At no time were these files readily accessible except under specific authority of a senior professional working on the project.

V. ADDITIONAL DATA OBTAINED FOR THE FOLLOW-UP STUDY

In addition to the information collected through the survey instrument, most of the results presented in this report relied on data that were obtained from other sources. Specifically data were obtained from three sources: (a) the base-year student master file (containing data collected in the base-year study), (b) a PSE institutional characteristics file (compiled by CEEB), and (c) current files of UB and Special Services to Disadvantaged Students (SSDS) projects maintained by DSSVP. The procedures whereby these data were merged with data collected through the first follow-up questionnaire are described in Appendix D; the purpose of this section is to describe briefly the information collected from these sources.

The base-year student master file was created during the base-year study and contained information about the study members as of that time. Of particular interest from that file were data relating to the initial sampling parameters concerning each student (including the raw sampling weights), the previous response history of each individual, an encrypted ID number (through which current data could be merged with extant data), individual background characteristics (age, race, sex, poverty classification, and academic risk classification), as well as educational and UB participation status at each of three points in time (fall 1973, spring 1974, and fall 1974). This source was available on computer tape at RTI and is documented elsewhere.[10]

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[10] Research Triangle Institute. Upward-Bound Student Master Data File: Standard Codes and Tape Layout. Research Triangle Park, N.C.: Research Triangle Institute, December 1975.

The CEEB PSE institutional source file was created from four sources: (a) the 1973-74 Higher Education Directory, (b) the 1973-74 Tripartite Application Data File, (c) the 1972-73 HEGIS Finance Survey, and (d) the 1972 ACE Institutional Characteristics file. This file was subsequently used to create the institutional indices used in this study (see Appendix D). The file was obtained by RTI from CEEB and is documented separately.[11]

Information obtained from DSSVP files was restricted to names and addresses of all PSE institutions that hosted either a UB project or an SSDS project during any of the academic years 1974-75, 1975-76, or 1976-77. This information was used in determining institutional host status, for each of the two programs, for all institutions in the CEEB institutional source file.

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[11] Tenison, L. J. Description and Specifications for the Linked NLS-Institutional Data Base. Washington, D.C.: College Entrance Examination Board, March 1976.

Appendix D

Data Management and Processing Procedures

Appendix D

Data Management and Processing Procedures

I. GENERAL

This appendix describes the procedures followed in management and processing of data collected for the first follow-up of the national evaluation of the Upward Bound (UB) program. The operations involved concern, in the main, automated machine processing, data files, and systems that were used to facilitate both the collection of data and the preparation of those data for statistical analysis. The major tasks involved in data management and processing can be divided into two distinct but related areas--survey support and data base construction. The term survey support defines a broad range of data management activities directed toward providing control and aid to other survey operations. Tasks that fell within the realm of survey support were: (a) address file maintenance, (b) sample selection, (c) mailing label production, (d) receipt control, and (e) periodic status reporting. Although these tasks can be handled manually, and have been in many surveys, it was felt that in order to produce a survey of high quality a computerized system of control over survey operations was necessary. The area of data base construction involved: (a) preparation and reformatting of the base-year data, (b) editing and merging of the follow-up data, (c) construction and merger of postsecondary school indices, and (d) construction of analysis variables.

Each of these areas are treated separately in the various sections of this appendix to provide detailed descriptions of the major steps that led to the data base upon which this report rests. Additionally, descriptions of the quality of data collected or obtained from external sources are presented. The seven sections that follow are: (a) survey support, (b) data base construction, (c) editing the follow-up questionnaire, (d) construction of institutional indices, (e) construction of analysis variables, (f) data quality, and (g) final data base format and description. The first two following sections present general descriptions of procedures and policies; the remaining sections detail the major aspects of the construction process and present tabular data specifying the exact procedures and specifications used throughout the follow-up study. Much of the notation used in this appendix allows direct and precise translation into machine-readable specifications for the particular operations involved.

II. SURVEY SUPPORT

It has been RTI's experience that the day-to-day operations of a survey are best monitored by a combination of automated and manual processes. Over several years of survey operation a set of procedures and computer software has been developed that allows accurate and timely survey monitoring. This type of survey management has proved to be virtually essential in surveys that have several waves of mailouts, follow-ups, and that respond dynamically to prior events. The entire follow-up survey was monitored by such a computerized control system from time of first mailing until an edited machine-readable data base was established. Figure D.1 describes the data sources that were inputs to the survey system, the products that were its outputs, and the interrelations of the various data sets. The basic logic underlying this control system was that all operations would pass through one central computer-controlled process prior to any further action and that the central process would verify, edit, validate, and schedule activities using the totality of information available at any given time. Table D.1 lists the basic operations performed by the survey control system.

The system was designed to respond to the occurrence of events and to monitor the sequence and relationship of events as they occurred over time. In general, the system could access data that reflected the current survey status of each sample member (survey status file) as well as useful historical information describing the sample member's sequencing through each of several defined survey events (event code file). To be effective, the system required input for each defined event that took

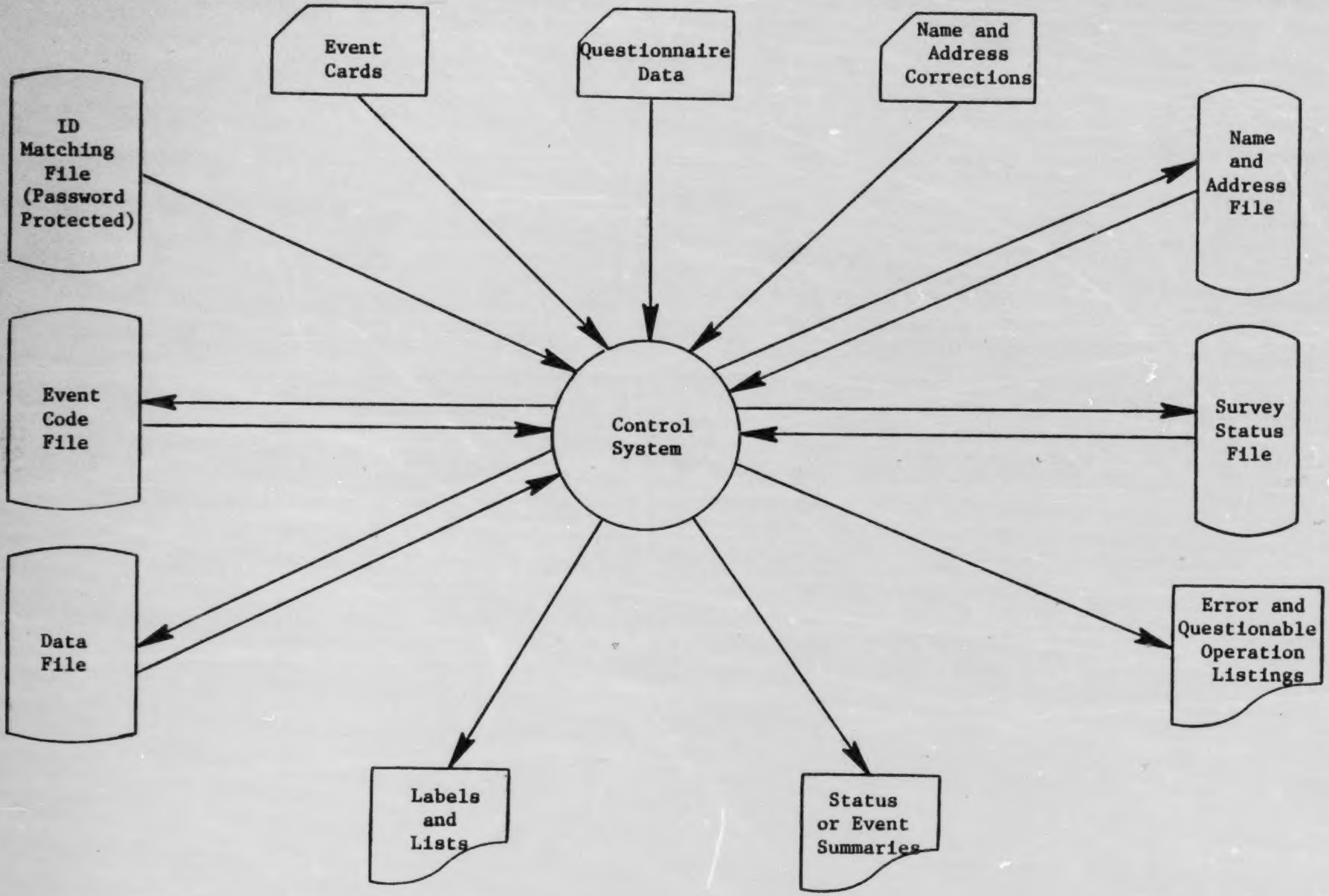
Event Cards
Questionnaire Data
Name and Address Corrections
ID Matching File (Password Protected)
Name and Address File
Event Code File
Control System
Survey Status File
Data File
Error and Questionable Operation Listings
Labels and Lists
Status or Event Summaries

Figure D.1. Study data set elements in relation to survey control system.

Table D.1

BASIC OPERATIONS PERFORMED BY SURVEY CONTROL SYSTEM

Operation	Input Data Sets	Output Operations
1. Generate Mailing Labels	Name and Address File ID Matching File Survey Status File	Print List and Mailing Labels Update Survey Status File Update Event Code File
2. Process Events	Event Cards ID Matching File Survey Status File	Print Event Errors (Not Processed) Print Questionable Events Update Survey Status File Update Event Code File
3. Update Addresses	Name and Address Correction Cards ID Matching File Survey Status File	Print Attempted Illegal Update (Not Processed) Update Name and Address File Update Survey Status File Update Event Code File
4. Sample for Telephone Interview	Name and Address File ID Matching File Survey Status File	Print Lists and Labels Update Survey Status File Update Event Code File
5. Produce Status Summaries	Survey Status File	Print Summary
6. Produce Event Summaries	Event Code File	Print Summary
7. Edit and Enter Questionnaire Data	Questionnaire Data Cards ID Matching File Survey Status File Data File	Print Attempted Illegal Entry (Not Processed) Print Fail Edit Listing Enter Edited Data in Data File Update Survey Status File Update Event Code File

place; therefore, as any of the events occurred for an individual, the event was coded, keypunched, and entered into the system (except in the case of computer generated events, for which no additional input was necessary). Since the processing of these events was performed on a flow basis, they were reflected in the system within a few days of their occurrence. The specific events considered were various outcomes or activities that were to be monitored as listed in Table D.2. For each event a specific set of procedures was applied and the survey status file was appropriately updated to allow quantitative monitoring of events and accurate processing of subsequent events.

As events occurred, a card file of pending events was accumulated. A computer program processed the event cards to update the survey status vector of any involved individual in the survey. An individual's status vector contained nine elements, which are defined in Table D.3. There were 6,050 such vectors, one for each person in the data base. Not all events required card preparation since some events were computer-generated (see Table D.2).

In addition to providing a convenient and accurate record of survey progress, the support system allowed rigorous quality control of data management activities through a series of internal checks. Such checks involved accessing an individual's current status prior to performing various data entry operations, which considerably reduced the likelihood of entering data for the wrong individual and virtually eliminated the problem of duplicate record entry. All status vector elements, except element 0, were initialized to zero. Element 0 was set to zero for those individuals who were determined ineligible for the follow-up survey (see Appendix E). Given these initial parameters, a set of rules were defined for updating each element in response to the occurrence of specific patterns of events. Let V(i,j) indicate status vector element i for person j (i = 0, 1, ..., 9; j = 1, 2, ..., 6050). Element 0, (V(0,j)), contained a 0,1 code to indicate survey eligibility status (see Table D.3). All other event processing was preceded by a check of element 0. If, for any event encountered, V(0,j) had a zero value, then

Table D.2

MONITORED STUDY EVENTS

Event	Description
1[a]	First mailout of instrument (generated by computer at time of label generation).
2	First mailout returned by Post Office as undeliverable.
3	First mailout returned by sample member.
4	Forwarding address correction returned by post office (first or second mailout).
5[a]	Address correction made to name and address file (first or second mailout).
6[a]	Second mailout of instrument (generated by computer at time of label generation).
7	Second mailout returned by sample member.
8	Second mailout returned by Post Office as undeliverable.
9	Returned questionnaire failed scan edit (first or second mailout).
10	Returned questionnaire that previously failed edit was successfully resolved (first or second mailout).
11[a]	Selected for telephone survey (generated by computer at time of sample selection).
12	Telephone interview successfully completed for all key questionnaire items (1, 4, 5, 8).
13	Telephone interview completed but not all key questionnaire items were collected.
14	Data entered onto Master File (generated by computer at time of data entry).

[a] These events were generated by computer.

Table D.3

ELEMENTS OF A STUDY MEMBER'S SURVEY STATUS VECTOR

Element Number	Element Name	Valid Codes
0	Survey Eligibility Status	0 - Not in study 1 - In study
1	First Mailout Status	0 - Not mailed 1 - Mailed
2	First Mailout Return Status	0 - Not returned 1 - Returned by respondent 2 - Returned as undeliverable by post office 3 - Deceased
3	Address Status	0 - 1974 address 1 - Update available 2 - Update made
4	Second Mailout Status	0 - Not mailed 1 - Mailed
5	Second Mailout Return Status	0 - Not returned 1 - Returned by respondent 2 - Returned as undeliverable by post office 3 - Deceased
6	Scan Edit Status	0 - Passed edit 1 - Failed edit 2 - Failed and resolved
7	Telephone Sample Status	0 - Not selected 1 - Selected
8	Telephone Completion Status	0 - Not completed 1 - 100% completed (key items) 2 - Partially completed (key items) 3 - Deceased
9	Follow-Up Data Status	0 - Data not entered 1 - Data entered (source = mail 1) 2 - Data entered (source = mail 2) 3 - Data entered (source = telephone) 4 - Data entered (multiple sources)[a] 5 - Data lost

[a] In a few cases with duplicate returns from two or more survey waves, questionnaires were merged to get the most complete questionnaire.

an error condition[1] was raised. The specific additional actions and checks for each event, including the raising of error conditions, are given below.

(a) Event 1: First Mailout

```
    Update status vector element 1: V(1,j) = 1 if a mailing
label is produced for individual j.
```

(b) Event 2: Returned Mailout 1 as Undeliverable

```
    If V(1,j) = 1 and V(2,j) = 0, then V(2,j) = 2; else error.
```

(c) Event 3: Return of Mailout 1 from Respondent

```
    If V(1,j) = 1 and V(2,j) = 0, then V(2,j) = 1; else error.
```

(d) Event 4: Address Correction Received

```
    If (V(1,j) = 1 or V(4,j) = 1) and V(3,j) = 0,
then V(3,j) = 1;
    Else if (V(1,j) = 1 or V(4,j) = 1) and V(3,j) ≠ 0,
then print;[2]
    Else error.
```

(e) Event 5: Address Correction Made

```
    If V(1,j) = 1 or V(4,j) = 1, then DO;
        If V(3,j) = 2, then print;
        If V(3,j) = 1, then V(3,j) = 2;
        Else error;
        END;
    Else error.
```

(f) Event 6: Second Mailout

```
    V(4,j) = 1 if a second mailing label is produced for
individual j.
```

(g) Event 7: Return of Mailout 2 from Respondent

```
    If V(4,j) = 1 and V(5,j) = 0, then V(5,j) = 1;
    Else error.
```

---

[1] In general, the detection of an error resulted in an event being rejected to an error file and a printed message specific to the event.

[2] In general, the raising of the print condition resulted in an event-specific information message being printed and no further action in response to the event record.

(h) Event 8: Returned Mailout 2 as Undeliverable

```
If V(4,j) = 1 and V(5,j) = 0, then V(5,j) = 2;
Else error.
```

(i) Event 9: Questionnaire Fails Scan Edit

```
If V(2,j) = 1 or V(5,j) = 1, then DO;
    If V(2,j) = 1 and V(5,j) = 1, then print;
    If V(6,j) = 0, then V(6,j) = 1;
    Else error;
    END;
Else error.
```

(j) Event 10: Fail Edit Resolution

```
If V(2,j) = 1 or V(5,j) = 1, then DO;
    If V(2,j) = 1 and V(5,j) = 1, then print;
    If V(6,j) = 2, then error;
    If V(6,j) = 0, then error;
    Else V(6,j) = 2;
    END;
Else error.
```

(k) Event 11: Selection for Telephone Sample

```
V(7,j) = 1 if individual j is selected.
```

(l) Event 12: Full Telephone Completion of Questionnaire[3/]

```
If V(7,j) ≠ 1, then error;
If V(2,j) = 1 or V(5,j) = 1, then print;
If V(8,j) = 0, then V(8,j) = 1;
Else error.
```

(m) Event 13: Partial Telephone Completion of Questionnaire

```
If V(7,j) ≠ 1, then error;
If V(2,j) = 1 or V(5,j) = 1, then print;
If V(8,j) = 0, then V(8,j) = 2;
Else error.
```

(n) Event 14: Data Entered on Master File

```
If V(9,j) ≠ 0, then error;
Else V(9,j) = m, where 1≤m≤5 was determined and passed
by the data editing software (see Section VIII).
```

[3/] Full completion referred only to the four key items of the questionnaire, items 1, 4, 5, and 8 (see Appendix B).

III. OVERVIEW OF DATA BASE CONSTRUCTION

The data base construction tasks for the UB Follow-Up Study involved several merging and construction steps. Figure D.2 outlines the steps that led to the final UB data file. Two parallel construction efforts began at the initiation of the contract--construction of a basic student-oriented file from base year data on which to build during subsequent operations (steps A-C in Figure D.2) and construction of an institution-oriented file for later merging with the student data (Steps I-N in Figure D.2). After these two basic files were constructed, the questionnaire data was added and the student and institutional data were merged to produce the final data file (Steps D-H in Figure D.2). Finally, analytic indices and various weights were constructed, and the file was put in final form (Steps O and P in Figure D.2).

The final file resulting from this construction process is specified in Section VIII of this appendix, in which a detailed file layout is presented labeling each data element and listing the valid codes possible for that element. Details of the steps involved in producing the final data file and analytic indices are presented in the three sections immediately following. Computation involved in constructing sampling weights are detailed in Appendix E.

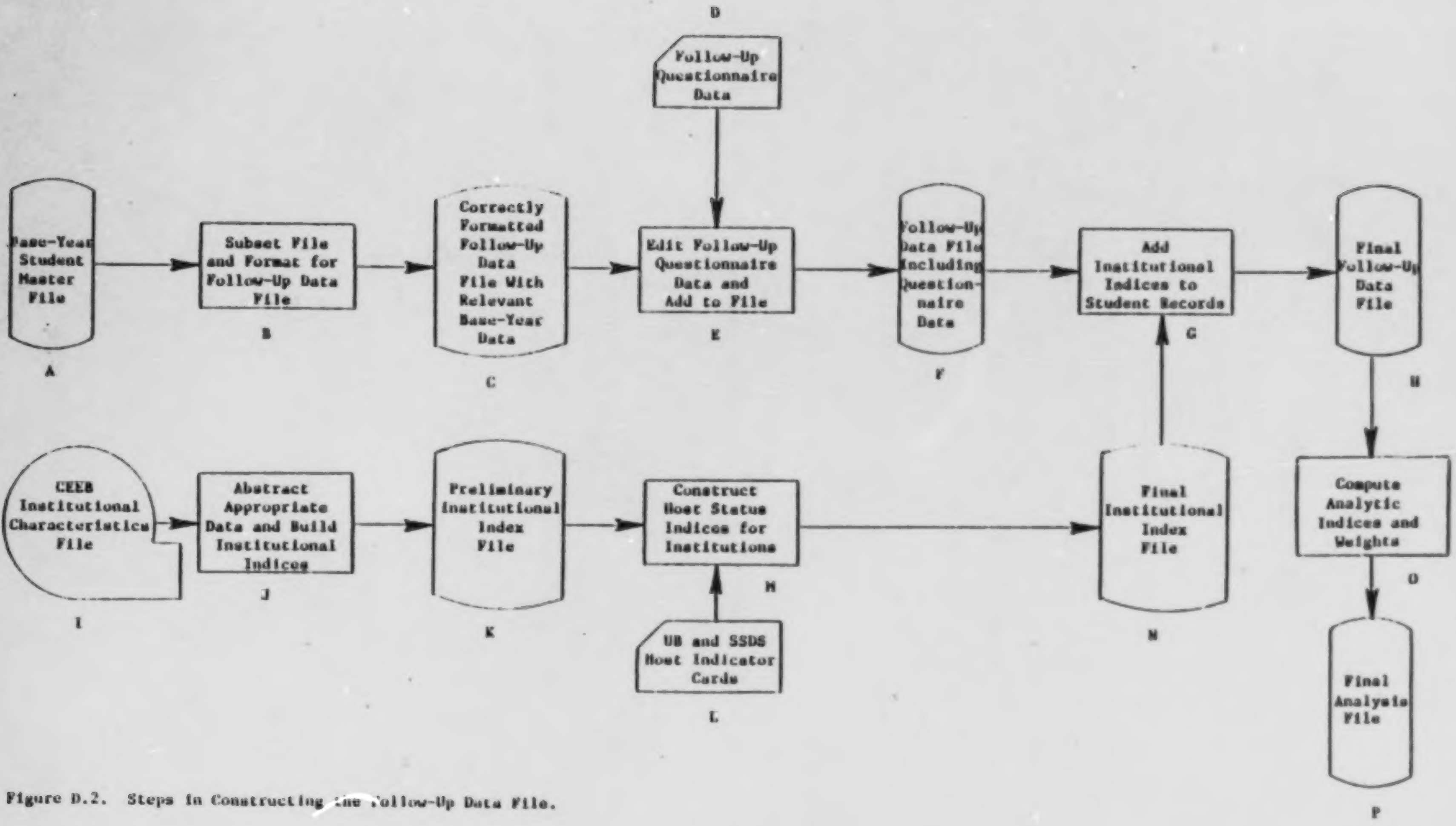
D
Follow-Up Questionnaire Data
Base-Year Student Master File
A
Subset File and Format for Follow-Up Data File
B
Correctly Formatted Follow-Up Data File With Relevant Base-Year Data
C
Edit Follow-Up Questionnaire Data and Add to File
E
Follow-Up Data File Including Questionnaire Data
F
Add Institutional Indices to Student Records
G
Final Follow-Up Data File
H
CEEB Institutional Characteristics File
I
Abstract Appropriate Data and Build Institutional Indices
J
Preliminary Institutional Index File
K
Construct Host Status Indices for Institutions
M
UB and SSDS Host Indicator Cards
L
Final Institutional Index File
N
Compute Analytic Indices and Weights
O
Final Analysis File
P

Figure D.2. Steps in Constructing the Follow-Up Data File.

IV. EDITING THE FOLLOW-UP QUESTIONNAIRE

Appendix C details the processing of the questionnaires prior to machine edit. This section will describe the process and procedures followed in machine editing the instrument after the data had been keypunched.

Once an instrument had been keypunched, several types of checks were computed before entering it into the data file. These checks included: (a) checks on ID number for range and check digit, (b) checks of survey status vectors, (c) range checks on questionnaire responses, (d) checks on FICE code for range and check digit, and (e) within- and between-instrument checks. If an individual record survived all of the below listed checks, then the data status vector element (V(9,j)) was set to a value of 1 to 3[4/] depending upon the type of questionnaire and the merged instrument datum. Simultaneously the data record was entered into the file. If the record failed any check, it was rejected and examined by hand. If possible, the errors were corrected and it was resubmitted to the editing process. Each of the tests and checks to insuring high data quality are discussed separately below.

A. ID Number Checks

All student ID's were checked to verify that they were in the range 1 to 6050. Additionally, the alphabetic check digit was verified.

B. Survey Status Checks

The survey status vectors were tested prior to considering the instrument for further processing. These tests were:

(1) Eligible for survey?: If $V(0,j) \neq 1$, then Error.

(2) Was this instrument administered?:

Let RS = 1, 2, 3 from datum labeling instrument type (1 = mail 1; 2 = mail 2; 3 = telephone).[5/]

Let k = (RS-1)*3 + 1.
If V(k,j) = 0, then Error.

[4/] Values of 4 and 5 were also entered in V(1,j) for special conditions (see Section VIII).

[5/] Merged instruments (see Table D.3) were coded as either 1, 2, or 3 as appropriate with an additional datum indicating that merger had been accomplished.

(3) Has data been received?:

If RS = 1, then if $V(2,j) \neq 1$, then Error.

If RS = 2, then if $V(5,j) \neq 1$, then Error.

If RS = 3, then if $V(8,j) = 0$ or $V(8,j) = 3$, then Error.

(4) Has any previous data been entered for this person?:

If $V(9,j) > 0$, then Error.

C. Other Editing and Consistency Checks

If an instrument passed the checks in A and B, then it was eligible for editing and consistency checking. There were several levels of these checks.

(1) Character code conversions. The punched data contained standard alphabetic codes for certain responses; they were recoded as follows: (a) bad data (BD) = -2, (b) no answer (NA) = -1, (c) multiple response (MR) = -2, and (d) refusal (RE) = -7.

(2) Verify range and check digit of FICE code; if not valid, reject the instrument.

(3) Check ranges of each item (see Table D.4).

(4) Perform consistency checks as detailed in Table D.5.1 and D.5.2.

Table D.4

RANGE CHECKS FOR FOLLOW-UP QUESTIONNAIRE ITEMS

Follow-Up Item	Valid Range[a]
1	1-10
2	1-3
3	1-3
4	1-3
5	999-13022
6	1-5
7.1	0-1
7.2	0-1
7.3	0-1
7.4	0-1
7.5	0-1
7.6	0-1
8.1	0-1
8.2	0-1
8.3	0-1
8.4	0-1

NOTE: For any value falling outside the specified range; the out-of-range data was replaced with -4.

[a] Excluding the valid negative code values.

Table D.5.1

WITHIN-INSTRUMENT DATA INCONSISTENCY PATTERNS

Inconsistencies Within 1977 Follow-Up Instrument[a]	
Item 8 Inconsistent Pattern I8.1 = 1 and any of {I8.2, I8.3, or I8.4} = 1	Action Label I8.1 through I8.4 with "8 code"[b]
Items 1 and 6 Inconsistent Pattern I6 = 1 and I1 $\geq$ 4 I6 = 2 and I1 $\geq$ 9	Action Label I1 and I6 with "8 code"[b]
Items 1 and 5 Inconsistent Pattern I1 $\geq$ 4 and I5 is high school code	Action Label I1 and I5 with "8 code"[b]

NOTE: The items of the follow-up questionnaire are referenced as In or In.m, where n is the number of the item as it appeared on the instrument and where m is the response alternative for those items allowing more than one response (m corresponds to the order, top to bottom, in which the response alternatives were presented on the instrument). See Appendix B for instrument.

[a] These checks were also performed during "scan edit" of the returned questionnaires, and if the inconsistencies were detected, the respondent was telephoned in an attempt to resolve them. In the event that some inconsistencies were not detected, could not be resolved (telephone contact could not be made), or were created through keying, then the actions specified in this section were implemented.

[b] An "8 code" is an 8 placed in the second byte of a data field. It was placed so that the original datum remained in the field but now contained a leading 8 to flag it. For example, in a 4-wide data field, such as I8.1-I8.4, if the indicated check failed (suppose I8.1-I8.4 took on values as below), then the flagged values would be as follows:

Value of Item	Old	Flagged
I8.1	_ _ _ 1	_ 8 0 1
I8.2	_ _ _ 0	_ 8 0 0
I8.3	_ _ _ 0	_ 8 0 0
I8.4	_ _ _ 1	_ 8 0 1

Table D.5.2

BETWEEN-INSTRUMENT DATA INCONSISTENCY PATTERNS

Inconsistencies Between 1977 Follow-Up Instrument and Previously Collected Data

A. Conditions for Performing Inconsistency Checks

If any of the 1977 instrument items to be checked were internally inconsistent (8 codes), the between-instrument checks for that item were not performed. If any base-year data elements carried a data quality code of 2, 4, 6, 8, 12, 14, 16, 18, 22, 24, 26, or 28, the instrument checks were not performed. If base-year data elements carried a data quality code of 10 or 20, then the data quality code was stripped and the check was performed.

B. Item 8

1. Questionnaire External Data Used in Inconsistency Checks

    a. Data Utility Digit (DUD) from 1974 Master File
    b. Decrypted student identification number (ID)
    c. Upward Bound participation index (UBPART) from 1974 data analysis file

2. Inconsistency Checks

    a. If originally sampled as a comparison student (ID.6 = 2), then

        (1) If DUD = 9, then I8.1 = 1 is inconsistent.
        (2) Otherwise, (DUD ≠ 9)

            (a) If UBPART = 7, I8.2 = 1 is inconsistent.
            (b) If UBPART = 9, I8.2 = 1 or I8.3 = 1 is inconsistent.

    b. If originally sampled as a UB student (ID.6 = 1) and if DUD = 0, then

        (1) If UBPART = 1, then I8.2 = 0, I8.3 = 0, or I8.4 = 0 are inconsistent.
        (2) If UBPART = 2, then I8.2 = 1, I8.3 = 0, or I8.4 = 0 are inconsistent.
        (3) If UBPART = 3, then I8.2 = 1; I8.3 = 1, or I8.4 = 0 are inconsistent.
        (4) If UBPART = 5, then I8.2 = 0 or I8.3 = 0 are inconsistent.
        (5) If UBPART = 6, then I8.2 = 1 or I8.3 = 0 are inconsistent.
        (6) If UBPART = 8, then I8.2 = 0 is inconsistent.

3. Action to be Taken for Inconsistencies

If inconsistencies were discovered, all of item 8 response alternatives and UBPART were labeled with a "10 code"; that is, a "10" was placed in bytes 1 and 2 of each data field.

(continued)

C. Items 1 and 5

1. Questionnaire External Data Used in Inconsistency Checks

   a. Fall 1974 PSE Enrollment Status (PSE74F) from 1974 master file.
   b. Fall 1974 Highest Grade Completed (HG74F) from 1974 master file.
   c. Spring 1974 Highest Grade Completed (HG74S) from 1974 master file.
   d. Fall 1973 Highest Grade Completed (HG73) from 1974 master file.
   e. Fall 1974 High School Enrollment Status (HSE74F) from 1974 master file.

2. Intermediate Computations

Determine from HG73, HG74S, and HG74F, the last recorded highest grade completed (HGC) as follows:

```
           If (HG74F is determinate), then HGC = HG74F;
Otherwise, If (HG74S is determinate), then HGC = HG74S;
Otherwise, If (HG73 is determinate), then HGC = HG73;
Otherwise, HGC is indeterminate.
```

3. Inconsistency Checks

   a. If (PSE74F = 1, 2, or 3), then I5 = high school code is inconsistent.
   b. If ((HGC = 12) or (HSE74F = 2)), then I1 < 3 is inconsistent.
   c. If (HGC = 11), then I1 = 1 is inconsistent.
   d. If (HSE74F = 1), then I1 = 10 is inconsistent.

4. Actions to be Taken for Inconsistencies

If any inconsistencies were determined, all inconsistent data elements (1977 questionnaire items <u>and</u> previous data items) were labeled with a "10 code" (in the event that the previous data item already carried a data quality code--see Section A above--then that code was restored).

V. CONSTRUCTING INSTITUTIONAL INDICES[6]

The original analysis which was anticipated in preparing the RTI proposal considered analysis of eight to ten straightforward institutional indices to be used in addressing analysis questions relating to postsecondary institutions (including UB and SSDS host status). On providing OPBE with a list of potentially available institutional characteristics from the CEEB institutional data file (the PSEF), the number of elements specified as desirable were far beyond the scope of analysis effort which had been considered by RTI. In an attempt to meet the desires of OPBE within the limited analysis budget for these analysis questions, an attempt was made to produce from the PSEF, nine meaningful institutional indices, which when combined with the UB and SSDS host status indices would provide a total of 11 institutional indices.

Ideally, the data elements from the PSEF would have been subjected to empirical scaling to develop indices that captured most of the variation in the data elements of interest. Such an approach was not adopted due to the limitation on funds available. Rather, an approach of a priori index construction was used. The nature of the indices is specified below. In cases where "cut points" were determined, this was accomplished jointly by RTI and OPBE after reviewing frequency distributions of the intermediate variables from which the indices were to be derived.

The sections that follow describe the construction process for each index. In general, index construction was a two-stage process. First, an intermediate variable was defined and then it was quantized to produce a categorical variable (the index).

A. Constructing Index of Institutional SES (ISES)

The variables in the data base for constructing this index were:

---

[6] The base data for constructing all these indices come from the data file specified in Tenison, L. J., Description and Specifications for the Linked NLS-Institutional Data Base, Washington, D.C.: CEEB Technical Report dated March 1976; and the variable numbers (Vn) cited herein refer to the variable number (n) given in that manuscript. The data for UB and SSDS host status were produced from USOE program records.

(1) V19--Estimated full-time undergraduates FY 74 in \$0-\$6,000 income range ($FTUG_1$).

(2) V20--Estimated full-time undergraduates FY 74 in \$6,000-\$9,000 income range ($FTUG_2$).

(3) V21--Estimated full-time undergraduates FY 74 in \$9,000-\$12,000 income range ($FTUG_3$).

(4) V22--Estimated full-time undergraduates FY 74 in over \$12,000 income range ($FTUG_4$).

(5) V18--Estimated total full-time undergraduates FY 74 (TFTUG).[7]

(6) V24--Estimated half-time undergraduates FY 74 in \$0-\$6,000 income range ($HTUG_1$).

(7) V25--Estimated half-time undergraduates FY 74 in \$6,000-\$9,000 income range ($HTUG_2$).

(8) V26--Estimated half-time undergraduates FY 74 in \$9,000-\$12,000 income range ($HTUG_3$).

(9) V27--Estimated half-time undergraduates FY 74 in over \$12,000 income range ($HTUG_4$).

(10) V23--Estimated total half-time undergraduates FY 74 (THTUG).[8]

Intermediate computations and variable construction for Institutional SES included the following steps:

(1) Construct for each category of income full-time equivalent student counts ($FTEUG_i$) as follows:

$$FTEUG_i = FTUG_i + (.5)HTUG_i.$$

(2) Construct total full-time equivalent students (TFTE) as follows:

$$TFTE = TFTUG + (.5)THTUG.$$

(3) Express each full-time equivalent student count within income category as a proportion (PUG_i) as follows:

$$PUG_i = FTEUG_i/TFTE, \ i \leftarrow 1, \ldots, 4.$$

---

[7] This is not necessarily equal to $\sum_{i=1}^{4} FTUG_i$.

[8] This is not necessarily equal to $\sum_{i=1}^{4} HTUG_i$.

The intermediate variable $PSES_i$, $i \leftarrow 1, 2, 3$, were computed then as follows:

(4) If any of the base variables were indeterminate, then each of $PSES_i$ was coded as -1. Further, if $(V19 + V20 + V21 + V22 > V18)$ or if $(V24 + V25 + V26 + V27 > V23)$, then each of $PSES_i$ was coded as -1.

(5) For each institution, compute 3 tentative variables reflecting proportion of FTE undergraduates in income levels less than $\$Y_i$ ($i \leftarrow 1, 2, 3$; Y_i = \$6,000, \$9,000, \$12,000). Define these tentative variables as:

$$PSES_i = \sum_{j=1}^{i} PUG_j .$$

The final index is computed from the intermediate variables of $PSES_1$ and $PSES_3$ as follows:

(1) If $PSES_1$ or $PSES_3$ is indeterminate,
then ISES = -1.

(2) If $((PSES_1 \text{ GE } .3)$ and $(PSES_3 \text{ LE } .5))$
then ISES = 1.

(3) If $((PSES_1 \text{ LT } .3)$ and $(PSES_3 \text{ GT } .5))$
then ISES = 3.

(4) Otherwise, ISES = 2.

Therefore, the final values of ISES are coded as follows:

$$\begin{array}{ll} 1 = \left.\begin{array}{l}\text{30\% or more of student body} \le \$6000 \\ \text{50\% or less of student body} \ge \$12000\end{array}\right\} & \text{(low SES)} \\ 2 = \text{not in 1 or 3} & \text{(mid-SES)} \\ 3 = \left.\begin{array}{l}\text{30\% or fewer of student body} < \$6000 \\ \text{50\% or more of student body} > \$12000\end{array}\right\} & \text{(high SES)} \end{array}$$

B. Constructing Index of Institutional Selectivity (ISEL)

The variables included in the data base for Institutional Selectivity were:

(1) V76--Median 1972 ACT score for institution (ACT).

(2) V77--Median 1972 MSQT score for institution (MSQT).

(3) V78--Median 1972 total SAT score for institution (TSAT).

Intermediate computations for ISEL included a conversion step for each of the data base measures to a common metric by expressing it as a standard score for each institution as follows:

$$Z_1 = \frac{\text{ACT} - \mu_1}{\sigma_1} ,$$

$$Z_2 = \frac{\text{MSQT} - \mu_2}{\sigma_3} ,$$

$$Z_3 = \frac{\text{TSAT} - \mu_3}{\sigma_3} ,$$

In computing the intermediate values, Z_i, the following rules were applied:

(1) If ACT is indeterminate, the Z_1 is indeterminate;

(2) If MSQT is indeterminate, the Z_2 is indeterminate;

(3) If TSAT is indeterminate, the Z_3 is indeterminate.

For the values of μ_i and σ_i, $i \leftarrow 1, 2, 3$, the empirical means and standard deviations of the distributions of ACT, MSQT, and TSAT were calculated.

From the common metric values, an average standard score for each institution was computed:

$$\text{ZSEL} = \frac{\sum_{i=1}^{3} Y_i Z_i}{\sum_{i=1}^{3} Y_i} ,$$

$$\text{where } Y_i = \begin{cases} 0, \text{ if } Z_i \text{ is indeterminate,} \\ 1, \text{ if } Z_i \text{ is determinate.} \end{cases}$$

The index ISEL was then computed as follows:

(1) If ZSEL is indeterminate (i.e., $Y_i = 0$ for all i),
then ISEL = -1

(2) If ((ZSEL GE -5) and (ZSEL LT -.5))
then ISEL = 1

(3) If ((ZSEL GE -.5) and (ZSEL LT .1))
then ISEL = 2

(4) If ((ZSEL GE .1) and (ZSEL LT .8))
then ISEL = 3

(5) If ((ZSEL GE .8) and (ZSEL LT 5.0))
then ISEL = 4

(6) Otherwise, ISEL = -1

C. Constructing Index of Institutional Costs (ICOST)

Only one variable from the data base was used to determine Institutional Costs:

V28[9]--Estimated weighted average cost per student for 1972-73 AY (WAC72).

The index ICOST was computed as follows:

(1) If WAC72 is indeterminate,
then ICOST = -1

(2) If (WAC72 LT 2000)
then ICOST = 1

(3) If ((WAC72 GE 2000) and (WAC72 LT 2500))
then ICOST = 2

(4) If ((WAC72 GE 2500) and (WAC72 LT 3000))
then ICOST = 3

(5) If ((WAC72 GE 3000) and (WAC72 LT 4000))
then ICOST = 4

(6) If (WAC72 GE 4000)
then ICOST = 5

D. Constructing Index of Institutional Size (ISIZ)

Three variables from the data base were used in constructing this index:

(1) V15--Full-time undergraduate students in fall 1972 from tripartite data (FTUG).

(2) V16--Half-time undergraduate students in fall 1972 from tripartite data (HTUG).

(3) V53--1972 resident degree credit undergraduates from HED (HEDSIZ)

---

[9] This value is reported for students needing and applying for aid only. It represents a composite of variables V48 - V52 over various student categories (e.g., dependent student living at home; self-supporting student; married with child; etc.).

An intermediate value of size was also computed from the tripartite data:

$$\text{TRISIZ} = \text{FTUG} + (.5)\text{HTUG}.$$

Undergraduate enrollment (SIZ) was then determined as follows:

```
(1)  If HEDSIZ is determinate
          then SIZ = HEDSIZ

(2)  Else, if TRISIZ is determinate
          then SIZ = TRISIZ

(3)  Else, SIZ = -1.
```

The index ISIZ was then computed as follows:

```
(1)  If SIZ is indeterminate,
          then ISIZ = -1

(2)  If (SIZ LT 500)
          then ISIZ = 1

(3)  If ((SIZ GE 500) and (SIZ LT 1000))
          then ISIZ = 2

(4)  If ((SIZ GE 1000) and (SIZ LT 3000))
          then ISIZ = 3

(5)  If ((SIZ GE 3000) and (SIZ LT 8000))
          then ISIZ = 4

(6)  If (SIZ GE 8000)
          then ISIZ = 5.
```

E. Constructing Index of Federal Aid Distribution (IFAD)

The variables used to construct this index were:

(1) V31--Estimated unduplicated count of students receiving EOG, CWS, or NDSL in AY 72-73 (NAID).

(2) TRISIZ--Full-time equivalent undergraduate enrollment, fall 72, from tripartite data (computed in Subsection V.D above as a combination of V15 and V16).

Intermediate computations involved expressing the number of students receiving aid as a proportion of total FTE student population at the institution (PSAID) as follows:

$$\text{PSAID} = \text{NAID}/\text{TRISIZ}$$

The index was computed from the intermediate variable PSAID as follows:

(1) If PSAID is indeterminate (i.e., TRISIZ is indeterminate)
    then IFAD = -1

(2) If (PSAID EQ 0)
    then IFAD = 0

(3) If ((PSAID GT 0) and (PSAID LT .15))
    then IFAD = 1

(4) If ((PSAID GE .15) and (PSAID LT .3))
    then IFAD = 2

(5) If (PSAID GE .3)
    then IFAD = 3

F. Constructing Index of Institutional Student Aid Support (ISAID)

The variables used to construct this index were:

(1) V74--Student aid grant expenses for FY 1972 (GREXP).

(2) SIZE--Number of FTE undergradaute students (computed in Subsection V.D above).

Intermediate computations involved computing aid per FTE student (FTEAID),

FTEAID = GREXP/SIZE.

The index was computed from the intermediate variable FTEAID as follows:

(1) If FTEAID is indeterminate
    then ISAID = -1

(2) If (FTEAID EQ 0)
    then ISAID = 0

(3) If ((FTEAID GT 0) and (FTEAID LT 50))
    then ISAID = 1

(4) If ((FTEAID GE 50) and (FTEAID LT 100))
    then ISAID = 2

(5) If ((FTEAID GE 100) and (FTEAID LT 900))
    then ISAID = 3

(6) If (FTEAID GE 900)
    then ISAID = 4

G. Constructing Index of Institutional Pacial Characteristics (IRACE)

One variable from the data base was used to construct this index:

V8--Predominant race of institution (RACE).

The index IRACE was equivalent to RACE

$(IRACE = RACE)$,

unless RACE is indeterminate, then $IRACE = -1$.

H. Constructing Index of Institutional Sex Characteristics (ISEX)

One variable from the data base was used to construct this index:

V10--Predominant sex of student body (SEX).

The index ISEX was equivalent to SEX

$(ISEX = SEX)$,

unless SEX is indeterminate, then $ISEX = -1$.

I. Constructing the Indices of Institutional Control (ICONT) and Institutional Type (ITYPE)

Two variables from the institutional data file were used to construct these two indices:

(1) V9--HED control/type of institution (CONTYP)

(2) V12--CEEB type specification

The index ICONT was constructed as follows:

For $i \leftarrow 1, 2, 3$, and for determinate values of CONTYP,
if $(CONTYP \leq X_i)$, then $ICONT = i$,
where $X_1 = 19$, $X_2 = 26$, $X_3 = 27$.

If CONYTP was indeterminate, then the index was constructed from V12 as follows:

If $(V12 \geq 8)$ or $(V12 \leq 0)$, then $ICONT = -1$;
Otherwise, for $i \leftarrow 1, 2, 3$,
    If $(V12 \leq W_i)$, then $ICONT = i$,
    where $W_1 = 3$, $W_2 = 6$, $W_3 = 7$.

ITYPE was constructed as follows:

```
For $i \leftarrow 1, 2, 3$, and determinate values of CONTYP,
if (CONTYP $\varepsilon\ \chi_i$), then ITYPE = i,
where $\chi_i$ = {13, 16, 23, 26, 27}, $\chi_2$ = {12, 15, 22, 25},
      $\chi_3$ = {11, 14, 21, 24}.
```

If CONYTP was indeterminate, then the index was constructed from V12 as follows:

```
If (V12 = 0) or (V12 = 9), then ICONT = -1;
Otherwise, for $i \leftarrow 1, 2, 3$,
If (V12 $\varepsilon\ \chi_1$), then ITYPE = i,
where $\chi_1$ = {3, 6, 7, 8}, $\chi_2$ = {2, 5}, $\chi_3$ = {1, 4}.
```

VI. CONSTRUCTION OF ANALYSIS VARIABLES

A set of 22 analysis variables were constructed. These variables are listed in Table D.6. The variables were constructed either by combining base year and follow-up data or by transforming follow-up data directly. The exact rules for variable construction are listed in Tables D.7 through D.16. Two variables are identical to the follow-up questionnaire items which defined them. These two variables, current military status (MIL) and deterrents to education ($EDDET_1$ through $EDDET_6$), are not treated in the tables. The index of current military status was defined by response to item 3 of the followup questionnaire as:

-1 = Indeterminant,
1 = Never in service,
2 = Now in service,
3 = Veteran.

The indices for deterrents to education ($EDDET_i$, $i = 1$ to 6) were defined by responses to items 7.1 through 7.6 of the followup questionnaire as:

-1 = Indeterminant,
1 = Condition i was not a deterrent,
2 = Condition i was a deterrent.

Throughout Tables D.7 through D.16, the items of the follow-up questionnaire are referenced as FU.n or FU.n.m, where n is the item number and where m is the response alternative for those items allowing more than one response (m corresponds to the order--top to bottom--in which the response alternatives were presented on the instrument).

As indicated in Section IV (Tables D.5.1 and D.5.2) and Section VIII, the final data file contains a system of consistency flags. Table D.17 lists the specific rules of defining indeterminate data when multiple variables were involved and when variables may have been flagged from prior consistency checking.

Table D.6

ANALYSIS VARIABLES

	Variable Name	Mnemonic
1.	Educational Progress	EDP
2.	Persistence through or beyond high school	EDHS+
3.	Persistence through PSE entry	PENS
4.	Progress in PSE given PSE entry	HG|PE
5.	Current enrollment in PSE given PSE entry	EN|PE
6.	Lag entry into PSE	LENT
7.	Current work status	WORK
8.	Current military status	MIL
9.	Educational aspirations	EDASP
10-15.	Deterrents to Education	$EDDET_1$ to $EDDET_6$
16.	Age in 1973	AGE
17.	Race	RACE
18.	Sex	SEX
19.	Grade in 1973	GL
20.	Academic risk status	RISK
21.	UB project number	UBPROJ
22.	Extent of participation in UB	UBPART

Table D.7

PRODUCING A MEASURE OF EDUCATIONAL PROGRESS (EDP)

Value of Follow-Up Question FU.1	Value of Follow-Up Question FU.5	Value of EDP[a]
Indeterminate or Inconsistent	(Any value)	-1
(Any value)	Indeterminate or Inconsistent	-1
1	High School	10
2	High School	11
3	High School	12
1,2,3, or 4	PSE Institution	12.5
5,7	PSE Institution	13
6,8	PSE Institution	14
9	PSE Institution	15
10	PSE Institution	16

[a] -1 = indeterminate or inconsistent.

Table D.8

PRODUCING AN INDEX OF PERSISTENCE IN EDUCATION THROUGH OR BEYOND HIGH SCHOOL (EDHS+)

Value of HSE74F		Value of EDP -1	Value of EDP 11 or Less	Value of EDP 12 or More
Indeterminate or inconsistent		-1	1	2
In HS	1	-1	1	2
Graduated	2	2	-1	2
Out, Not Grad.	3	-1	1	2
Out, Status ?	4	-1	1	2

NOTE: -1 = Indeterminate or inconsistent; 1 = Did not complete high school; 2 = Completed high school or higher.

Table D.9

PRODUCING AN INDEX OF PERSISTENCE IN EDUCATION THROUGH PSE ENTRY (PENS)

Value of PSE74F	Value of EDP		
	-1	12 or less	12.5 or more
Inconsistent or Indeterminate	-1	1	2
1	2	-1	2
2	2	-1	2
3	2	-1	2
4	-1	1	2
5	-1	1	2
6	1	1	2

NOTE: -1 = Indeterminate or inconsistent; 1 = Never entered PSE; 2 = Entered PSE.

Table D.10

PRODUCING AN INDEX OF PROGRESS IN PSE GIVEN PSE ENTRY (HG|PE)

Value of EDP	Value of HG|PE[a]
-1	-1
10, 11, 12	-3
12.5	12.5
13	13
14	14
15	15
16	16

[a] -1 = Indeterminate; -3 = Not applicable.

Table D.11

PRODUCING AN INDEX OF CURRENT ENROLLMENT IN PSE GIVEN PSE ENTRY (EN|PE)

	Value of FU.4		
Value of PENS	Indeterminate or Inconsistent	1	2,3
-1	-1	-1	-1
1	-3	-3	-3
2	-1	1	2

NOTE: -3 = Not applicable; -1 = Indeterminate or Inconsistent; 1 = Not currently enrolled; 2 = Currently enrolled.

Table D.12

PRODUCING AN INDEX OF LAG ENTRY INTO PSE FOR 1973 TWELFTH GRADERS WHO DID NOT ENTER PSE BY FALL 1974 (LENT)

	Value of PENS		
Value of PSE74F	-1	1	2
Indeterminate/ Inconsistent	-1	-1	-1
1	-3	-3	-3
2	-3	-3	-3
3	-3	-3	-3
4	-1	1	2
5	-1	1	2
6	-1	1	2

NOTE: -1 = Indeterminate or inconsistent; -3 = Not applicable; 1 = No lag entry; 2 = Lag entry. This index was computed only in the event that GL73 = 12; for GL 73 ≠ 12, the value of LENT was -3.

Table D.13

AN INDEX OF CURRENT WORK STATUS (WORK)

	Value of FU.3			
Value of FU.2	Indeterminate	1	2	3
Indeterminate	-1	-1	4	-1
1	1	1	4	1
2	2	2	4	2
3	3	3	4	3

NOTE: -1 = Indeterminate; 1 = Not working; 2 = Working, full time; 3 = Working, part time; 4 = Working and in the military.

Table D.14

PRODUCING A MEASURE OF EDUCATIONAL ASPIRATIONS (EDASP)

Value of FU.6	Value of EDASP
Indeterminate or Inconsistent	-1 = Indeterminate
1	1 = High school only
2	2 = Vocational/business school
3	3 = Some college
4	4 = 4-Year college
5	5 = Graduate/ Professional School

Table D.15

ANALYSIS VARIABLES FROM THE 1974 DATA BASE

Variable	Value	Value Label
AGE	-1	Missing or indeterminate
	14	14 or younger
	15-20	Actual age in years
	21	21 or older
RACE	-1	Missing or indeterminate
	1	Black
	2	White
	3	Other
SEX	-1	Missing or indeterminate
	1	Male
	2	Female
GL73	-1	Missing or indeterminate
	10-12	Actual grade
POVS	-1	Missing or indeterminate
	1	Poverty level
	2	Not poverty level
RISK	-1	Missing or indeterminate
	1	Academic risk
	2	Not academic risk
UBPROJ [a]	1-54	One to one transformation of project ID number

[a] This variable was subsequently recoded through criterion scaling (see Appendix G, Section IV).

Table D.16

PRODUCING A MEASURE OF EXTENT OF UB PARTICIPATION (UBPAR)

1974 Variable UBPART			Number of Grades in UB From FU.8[a]				
Value	Meaning						
Code	Grade Fall 73	Year Joined UB	Inconsistent/ Indeterminate	0	1	2	3
-1	--	--	-1	0	1	2	3
1	12	10	3	-1	1	2	3
2	12	11	2	-1	1	2	-1
3	12	12	1	-1	1	-1	-1
4	12	Never	-1	0	1	-1	-1
5	11	10	-1	-1	1	2	3
6	11	11	-1	-1	1	2	-1
7	11	Never	-1	0	1	2	-1
8	10	10	-1	-1	1	2	3
9	10	Never	-1	0	1	2	3

NOTE: -1 = indeterminate or inconsistent; 0 = never participated; 1 = 1 year of participation; 2 = 2 years of participation; 3 = 3 years of participation.

[a] Derived from FU.8 as follows:

Indeterminate if any of FU.8.i is indeterminate

$$\text{Otherwise Number of years} = \begin{cases} 0, \text{ if FU8.1} = 2 \\ k, \text{ otherwise, where} \\ \quad k = \sum_{i=2}^{4} (\text{FU8.}i-1) \end{cases}$$

Table D.17

DEFINITION OF INDETERMINATE DATA

1. EDP
    a. Ignore 10 flags on FU.1 or FU.5.
    b. If either FU.1 or FU.5 < 0, then it is indeterminate.
    c. If FU.5 is flagged with an 8, then both FU.1 and FU.5 are indeterminate.
    d. If FU.1 is flagged with an 8 and FU.5 is not flagged, then ignore flag on FU.1.
2. EDHS+
    a. HSE74 is indeterminate if it is flagged > 4 or if its value is < 0.
3. PENS
    a. If PSE74F is flagged > 4 or its value is < 0, then it is indeterminate.
4. HG|PE -- defined in Table D.10
5. EN|PE
    a. FU.4 is indeterminate if it is < 0.
6. LENT
    a. GL73: accept values of 12 or 412; values of 812, 1812, 2812 are not = 12.
7. WORK -- defined in Table D.13
8. EDASP
    a. If FU.6 is < 0 or flagged with an 8, it is indeterminate.
9. AGE - UBPROJ
    a. For these variables any value less than 0 or a flag > 4 is indeterminate.
10. UBPAR
    a. Ignore 10 codes of FU.8i.
    b. FU.8i is indeterminate if < 0 or flagged by an 8.
11. GL73
    a. GL73 is indeterminate if flagged > 4 or its value is < 0.

VII. DATA QUALITY

This section describes the quality of the data that were collected or constructed during the course of the follow-up study. While response rates and sample coverage define the upper limits of data quality, two related problems of data quality that are typically present in questionnaire surveys are: (a) partial data sets, introduced by incomplete, inconsistent, or otherwise indeterminate responses on returned questionnaires, and (b) questionable validity of responses. The various data processing checks and the data quality indicators used in this study (see Section VIII below) provided an excellent avenue for investigating data indeterminacy. The receipt of duplicate returns from a subset of individuals (due to overlapping data collection steps) provided a means of examining the reliability of responses.

The section is divided into three subsections, each dealing with a separate data quality problem. Subsection A discusses data obtained from the follow-up questionnaire and from the base-year data base; Subsection B discusses data derived from an external data file to construct institutional indices; and Subsection C presents the results of an investigation of follow-up data reliability.

A. Follow-Up Questionnaire Data Quality

The follow-up questionnaire consisted of eight items. Questionnaire data were collected by mail, telephone, and in some cases from sources other than students. Table D.18 details the resulting data quality, showing the extent of questionable or indeterminate data in the final data base. Data problems that are internal to the questionnaire (e.g., missing data and internally inconsistent responses), are seen to be well within normal limits; in fact, they are typically quite low relative to similar surveys. Given the manual fail edit checks for inconsistency and completeness and subsequent attempts at telephone resolution, the minimal nature of such problems was not surprising. Where incomplete or internally inconsistent data exist for the critical questionnaire items (1, 4, 5, and 8), they represent either unresolvable (inability to contact by telephone) fail-edit cases, refusals to provide the particular piece of information, or inability of a secondary source to provide the information.

Table D.18

EXTENT OF QUESTIONABLE OR INDETERMINATE DATA IN THE FINAL DATA BASE

Questionnaire Item	Number of Cases With Missing Data[a]	Number of Cases Showing Inconsistencies		Total Questionable or Indeterminate[b]
		Within Instrument	With Previous Data	
1	35	7	10	52 (1.2%)
2	179	0	0	179 (4.2%)
3	164	0	0	164 (3.9%)
4	138	0	0	138 (3.3%)
5	31[c]	4	83	118 (2.8%)
6	344	3	0	347 (8.2%)
7	74	0	0	74 (1.8%)
8	79	0	1,245[d]	1,324 (31.4%)

[a] This includes item nonresponse, "don't know" responses, and refusals. Such cases are much more frequent in questionnaires completed by telephone from secondary sources, but are also evident in mail responses and primary source telephone interviews.

[b] Percentages are expressed in relation to the 4,221 completed cases.

[c] This does not include a count of cases in which data for the PSE institution given are not available in the PSE data file.

[d] This figure is somewhat exaggerated in terms of actual UB participation status. Only 100 inconsistencies exist in which participation or nonparticipation is inconsistent. The remaining cases reflect inconsistencies between currently reported and previously determined extent of participation.

The telephone interviewers were instructed to collect as much questionnaire information as possible from secondary sources in their efforts to trace the primary source, but as long as there was still a reasonable chance in contacting the primary source, the secondary source data was not transcribed. At the point of data collection cutoff, there remained a relatively large number of pending cases in which secondary source data had been collected but for which the possibilities of primary source contact had not been completely exhausted. These are the cases that served to raise substantially the indeterminate data counts over the counts for data obtained directly from sample members.

Inconsistencies with the previously collected data exist only in the case of items 1, 5, and 8; primarily for item 8. Although it had not been anticipated that inconsistent data would be used in analysis, rethinking was necessary in light of the approximately 30 percent inconsistency rate for Upward Bound participation (item 8). On examining the nature of these inconsistencies, it was found that only 100 of the inconsistencies (less than 2.5 percent of the total completions) represented cases in which an individual previously classified as having participated in UB now stated never having participated. Of these 100 cases, 37 (less than 1 percent of the total completions) are indiviudals who were originally selected as being in a UB project in fall 1973, but who now state they never participated. Of the 37, only 2 had been identified as suspect during the base-year study (a total of 33 had been so identified previously). The remaining 63 cases are individuals who were originally (fall 1973) selected into the comparison group, but who had indicated (or a secondary source had indicated for them) in the fall 1974 data collection period, that they had participated in UB; they now state no prior participation in UB. Since all but 35 of these 100 cases were excluded from the base-year analyses, it is quite unlikely that previous findings, regarding comparisons of UB participants and nonparticipants, are compromised. Even though the data for all 100 cases have been flagged as inconsistent in the master file, the data for the 63 individuals in the nonparticipant group were used in analysis, on the assumption that they had been classified as nonparticipants on two of three occasions and that as such they were likely nonparticipants.

The remaining 1,145 cases represent reported UB participation patterns that are inconsistent with the previously derived index of extent of UB participation. It is unlikely that unreliability due to retrospective reporting is the sole contributor to this large inconsistency rate; rather it is suspected that the inconsistency is a reflection of the imprecision of the previously derived index (an index specifying grades in which participation occurred that was derived from reported date of first participation in the program). Even though the data for such cases have been flagged as inconsistent in the master file, these data were used in analysis on the basis of the previously stated hypothesis. It should be noted that the extent of this type of inconsistency tends to invalidate previously reported results comparing UB participants with varying degrees of exposure to the program.

Data availability of variables from the base-year file has been previously documented in the base-year report.[10] The concern in this follow-up study, however, is the availability of the relevant base-year data items among the 4,221 sample members for whom follow-up questionnaire data were obtained. Table D.19 presents the rates of available data, from base-year variables, for that subset of individuals. It is clear that the rates are quite satisfactory and in only one case do they fall below 95 percent. In the case of poverty status there appears to be differential data availability between former UB participants and nonparticipants, which was also evidenced for the full base-year data base.

B. Availability of PSE Institutional Data

A second data set consists of the data obtained on PSE institutions. As indicated previously, the PSE indices were derived from an external data file and, if data were unavailable, no further efforts were made to obtain them. There was a relatively large amount of indeterminate data in the external file, which translated to an indeterminacy in the institutional

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[10] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs). (Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-152)

Table D.19

AVAILABILITY OF MAJOR BASE-YEAR VARIABLES AMONG THOSE SAMPLE MEMBERS FROM WHOM FOLLOW-UP QUESTIONNAIRES WERE OBTAINED

Variable	Former Participant[a]		Nonparticipant[a]		Total	
	Number	Percentage	Number	Percentage	Number	Percentage
Age	2692	99.3%	1492	98.9%	4184	99.1%
Sex[b]	2712	100.0	1506	99.8	4218	99.9
Race	2683	98.9	1462	96.9	4145	98.2
Poverty Status	2382	87.8	1478	97.9	3860	91.4
Academic Risk Status	2693	99.3	1503	99.6	4196	99.4
Original Grade Cohort[b]	2712	100.0	1509	100.0	4221	100.0

NOTE: These data are based on the 4221 sample members (2712 former participants and 1509 nonparticipants) from whom a Follow-Up Questionnaire was received.

[a] Classification at time of base-year study

[b] Includes values that were imputed during the base year

indices constructed (see Section V above). The resultant indeterminacy of the PSE indices as reflected in the student data base is shown in Table D.20. It can be seen from the data presented that, with the possible exception of institutional selectivity (a variable that was unavailable for a very large number of institutions), the availability of institutional data in the student data base is sufficiently high for most analysis purposes, in most cases higher than had been expected. The indeterminacy of PSE data stems from three sources: (a) lack of determination as to PSE institution attended (N=35), (b) stated attendance at a PSE institution not represented in the institutional data file (N=316),[11] and (c) stated attendance at a PSE institution in the original data file for which the information was not available.

C. Data Reliability

Due to overlapping data collection activities, duplicate returns were received for several individuals.[12] These duplicate returns provided a natural vehicle for an examination of the reliability of the returned data. There were three available comparisons of interest: (1) comparison of duplicate mail returns, (2) comparison of mail returns with primary source information received by telephone, and (3) comparison of mail returns with secondary source information obtained by telephone. Theoretically, these three comparisons include increasing sources of unreliability: duplicate mail returns reflect only temporal instability; mail returns and primary-source telephone responses reflect both temporal instability and differential mode of administration (probably related to the greater explanatory power of telephone interviews and differential propensities to provide falacious data in the two modes); and mail return and secondary-source telephone responses reflect differential mode of administration and differential information source.

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[11] RTI and OPBE agreed jointly in May that no attempt would be made to obtain the scanty PSE information that could be obtained for these institutions.

[12] The subset of individuals with duplicate returns do not, of course, represent a random or necessarily representative sample of respondents.

Table D.20

AVAILABILITY OF POSTSECONDARY INSTITUTIONAL DATA

PSE Variable[a]	Data Availability[b]
Index of institutional SES	2,017 (75.3%)
Index of institutional selectivity	1,659 (61.9%)
Index of institutional costs	1,987 (74.2%)
Index of institutional size	2,242 (83.7%)
Index of institutional Federal aid distribution	2,031 (75.8%)
Index of institutional student aid support	2,070 (77.3%)
Index of institutional racial characteristics	2,215 (82.7%)
Index of institutional sex characteristics	2,215 (82.7%)
Index of institutional control	2,288 (85.4%)
Index of institutional type	2,322 (86.6%)
Index of institutional UB host status	2,644 (98.7%)
Index of institutional SSDS host status	2,644 (98.7%)

[a] These indices are defined in Appendix D, Section V.

[b] Percentages reported are relative to 2,679 cases (those reporting PSE attendance and those for whom PSE attendance is indeterminate). Of these cases, PSE institution attended was indeterminate for 35 (1.3% of total), and that number is reflected as a reduction of the data availability rate for all 12 PSE variables. Further, 316 cases (11.8% of total) reported attendance of PSE institutions that were not recorded on the RTI PSE data file; these cases are reflected in a reduction of data availability for the first 10 PSE variables.

Table D.21 presents tabular results of the three types of comparisons. The theoretical increasing unreliability is generally reflected in the figures presented in the table. Moreover, some items appear to be more reliable than others. Temporal stability of responses is quite good, with the exception of two items [7(c) and 7(d)] relating to financial deterrents to education. Differential response biases in different modes of administration is suggested for items 1, 6, 7(c), 7(d), and 7(f)--highest grade completed, educational expectations, and three of the items regarding educational deterrents. Since about half of the data are from mail returns and half from telephone, some error in analyses of these items is indicated. Secondary source data seem to be particularly weak for items 1, 2, 5, 6, 7(c), 7(d), and 8(b). This is a source of potential data error, but due to the fact that less than 7 percent of the data collected were from secondary sources, the impact of this error should not be great.

Table D.21

INDICES OF RELIABILITY OF FOLLOW-UP QUESTIONNAIRE DATA

Follow-Up Questionnaire Item Number	Relevant Statistics	Comparison		
		Mail 1 and Mail 2	Mail and Telephone (Primary Source)	Mail and Telephone (Secondary Source)
Item 1 (Highest Grade Completed)	Number of Respondents	67	53	28
	Percent Hits[a]	89	62	53
	Percent Similar Hits[b]	100	96	82
	Relational Statistic[c]	.88	.68	.69
	Uncertainty Coefficient[d]	.78	.53	.53
Item 2 (Currently Working)	Number of Respondents	66	53	23
	Percent Hits[a]	95	87	56
	Percent Similar Hits[b]	95	87	56
	Relational Statistic[c]	.94	.81	.36
	Uncertainty Coefficient[d]	.83	.61	.13
Item 3 (Military Service)	Number of Respondents	66	53	24
	Percent Hits[a]	100	100	91
	Relational Statistic[c]	1.0	1.0	.81
	Uncertainty Coefficient[d]	1.0	1.0	.62
Item 4 (Currently Enrolled)	Number of Respondents	67	53	22
	Percent Hits[a]	94	100	95
	Percent Similar Hits[b]	95	100	100
	Relational Statistic[c]	.75	1.0	.85
	Uncertainty Coefficient[d]	.69	1.0	.88
Item 5 (School last attended)	Number of Respondents	67	53	28
	Percent Hits[a]	95	92	68
	Relational Statistic[c]	.99	.97	.96
	Uncertainty Coefficient[d]	.99	.97	.86
Item 6 (Amount of School Expected to Complete)	Number of Respondents	67	52	14
	Percent Hits[a]	93	63	50
	Percent Similar Hits[b]	98	98	100
	Relational Statistic[c]	.84	.56	.70
	Uncertainty Coefficient[d]	.67	.48	.42
Item 7(a) (Problems: Poor Grades)	Number of Respondents	67	53	26
	Percent Hits[a]	92	89	80
	Relational Statistic[c]	.66	.20	.10
	Uncertainty Coefficient[d]	.39	.05	.03
Item 7(b) (Problems: High School Preparation)	Number of Respondents	67	53	26
	Percent Hits[a]	91	90	88
	Relational Statistic[c]	.71	.74	e
	Uncertainty Coefficient[d]	.43	.53	e

- continued -

Table D.21 (continued)

Follow-Up Questionnaire Item Number	Relevant Statistics	Comparison		
		Mail 1 and Mail 2	Mail and Telephone (Primary Source)	Mail and Telephone (Secondary Source)
Item 7(c) (Problems: Personal Finances)	Number of Respondents	67	53	26
	Percent Hits[a]	79	64	50
	Relational Statistic[c]	.59	.32	.09
	Uncertainty Coefficient[d]	.27	.08	.01
Item 7(d) (Problems: Financial Aid)	Number of Respondents	67	53	26
	Percent Hits[a]	85	75	46
	Relational Statistic[c]	.63	.43	.18
	Uncertainty Coefficient[d]	.33	.15	.03
Item 7(e) (Problems: Program Not Available)	Number of Respondents	67	53	26
	Percent Hits[a]	94	96	92
	Relational Statistic[c]	.75	e	e
	Uncertainty Coefficient[d]	.51	e	e
Item 7(f) (Problems: Family Responsibility)	Number of Respondents	67	53	26
	Percent Hits[a]	97	83	80
	Relational Statistic[c]	.89	.22	.36
	Uncertainty Coefficient[d]	.72	.05	.17
Item 8(a) (UB Participation: Never)	Number of Respondents	67	53	25
	Percent Hits[a]	97	98	92
	Relational Statistic[c]	.93	.95	.70
	Uncertainty Coefficient[d]	.78	.86	.43
Item 8(b) (UB Participation: Grade 10)	Number of Respondents	67	53	25
	Percent Hits[a]	91	90	64
	Relational Statistic[c]	.82	.80	.28
	Uncertainty Coefficient[d]	.51	.54	.06
Item 8(c) (UB Participation: Grade 11)	Number of Respondents	67	53	25
	Percent Hits[a]	94	90	92
	Relational Statistic[c]	.87	.77	.83
	Uncertainty Coefficient[d]	.66	.50	.64
Item 8(d) (UB Participation: Grade 12)	Number of Respondents	67	53	25
	Percent Hits[a]	91	90	92
	Relational Statistic[c]	.82	.81	.85
	Uncertainty Coefficient[d]	.57	.56	.67

Table D.21 (continued)

NOTE: The figures reported in the table Indices of Reliability of Follow-Up Questionnaire Data represent unweighted comparisons of cases for whom duplicate returns were obtained. As such, they do not represent a random sample of respondents, nor are they necessarily representative of all respondents.

[a] This figure represents the percentage of cases for which identical responses were provided in the duplicated data.

[b] This figure represents the percentage of cases for which identical or similar responses were provided in the duplicated data (e.g., working full time and working part time; completed one year of college and completed two years of college). This percentage is only provided for appropriate items (e.g., items 1, 2, 4, 6).

[c] Phi coefficient for 2x2 tables; Cramer's V for tables greater than 2x2.

[d] The uncertainty coefficient (symmetric) measures the proportional reduction in uncertainty which is gained by knowing the joint distribution of cases in addition to the marginal distributions.

[e] Crosstabulations are 1x2 making measures of association inappropriate.

VIII. FINAL DATA BASE FORMAT

The data base format, which constitutes the remainder of this appendix, describes the variables, definitions, location, and codes for the Upward Bound follow-up data base. It represents the final product of the steps that have been discussed in Sections II through VI.

UPWARD BOUND FOLLOW-UP EVALUATION ANALYSIS FILE

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
1	ID	1-15	Encrypted ID Number	
		16-37	Blank Field	
13	RESP1	38-39	STF(A) Response Code 1	0 - Eligible nonrespondent (not refusal) 1 - Eligible nonrespondent (student refusal) 2 - Eligible nonrespondent (school refusal) 3 - Eligible nonrespondent (instrument non administered due to previous refusal status) 4 - Not eligible 5 - Not sampled (used with (FSQ(--AT) only) 6 - Eligible respondent (with available data) 7 - Eligible respondent (without available data) 8 - Noneligible respondent (responded to wrong form of FSQ) 9 - Eligible nonrespondent (with comparable data available through completion of wrong form of FSQ)
14	RESP2	40-41	STF(B) Response Code 2	
15	RESP3	42-43	BSQ(A) Response Code 3	
16	RESP4	44-45	BSQ(B) Response Code 4	
17	RESP5	46-47	D/TQ(A) Response Code 5	
18	RESP6	48-49	D/TQ(B) Response Code 6	
19	RESP7	50-51	D/TQ(C) Response Code 7	
20	RESP8	52-53	FSQ(UBAM) Response Code 8	
21	RESP9	54-55	FSQ(UBBM) Response Code 9	
22	RESP10	56-57	FSQ(UBAT) Response Code 10	
23	RESP11	58-59	FSQ(UBBT) Response Code 11	
24	RESP12	60-61	FSQ(CSAM) Response Code 12	
25	RESP13	62-63	FSQ(CSBM) Response Code 13	
26	RESP14	64-65	FSQ(CSAT) Response Code 14	
27	RESP15	66-67	FSQ(CSBT) Response Code 15	
28	SEXSC	68	Sex Source Code⁺	0-6
29	SEXF	69-70	Sex Flag*	0, 2
30	SEX	71-72	Sex Code	-1 - Indeterminate 1 - Male 2 - Female
31	RACESC	73	Race Source Code⁺	0-6
32	RACEF	74-75	Race Flag*	0, 10
33	RACE	76-77	Race Code	-1 - Indeterminate 1 - Black 2 - White 3 - Other

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
34	AGESC	78	Age Source Code+	0-6
35	AGEF	79-80	Age Flag*	0, 10
36	AGE	81-82	Age	-1 - Indeterminate 14 - 14 or younger 15 - 15 16 - 16 17 - 17 18 - 18 19 - 19 20 - 20 21 - 21 or older
37	POVSSC	83	Poverty Status Source+	0-6
38	POVSF	84-85	Poverty Status Flag*	0, 2, 4, 10
39	POVS	86-87	Poverty Status Code	-1 - Indeterminate 1 - Poverty level 2 - Not poverty level
40	UBPARSC	88	Extent of UB Participation Source+	0-6
41	UBPARF	89-90	Extent of UB Participation Flag*	0, 4, 10
42	UBPAR	91-92	Extent of UB Participation Code	-1 - Indeterminate 0 - None 5 - Less than 1 year 15 - 1-2 years 25 - 2-3 years 35 - 3-4 years 45 - 4-5 years 55 - 5 years or more
43	RISKSC	93	Academic Risk Source+	0-6
44	RISKF	94-95	Academic Risk Flag*	0, 4, 14
45	RISK	96-97	Academic Risk Code	-1 - Indeterminate 1 - Academic risk 2 - Not academic risk

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
46	HSE73SC	98	HS Enrollment (F73) Source+	0-6
47	HSE73F	99-100	HS Enrollment (F73) Flag*	0, 2, 4, 8, 18
48	HSE73	101-102	HS Enrollment (F73) Code	-1 - Indeterminate 1 - In HS 2 - Out of HS (graduated) 3 - Out of HS (not graduated) 4 - Out of HS (graduate status unknown)
49	HSE74SSC	103	HS Enrollment (S74) Source+	0-6
50	HSE74SF	104-105	HS Enrollment (S74) Flag*	0, 2, 4, 8, 10, 18
51	HSE74S	106-107	HS Enrollment (S74) Code	-1 - Indeterminate 1 - In HS 2 - Out of HS (graduated) 3 - Out of HS (not graduated) 4 - Out of HS (graduate status unknown)
52	HSE74FSC	108	HS Enrollment (F74) Source+	0-6
53	HSE74FF	109-110	HS Enrollment (F74) Flag*	0, 2, 4, 8, 10, 18
54	HSE74F	111-112	HS Enrollment (F74) Code	-1 - Indeterminate 1 - In HS 2 - Out of HS (graduated) 3 - Out of HS (not graduated) 4 - Out of HS (graduate status unknown)
55	GL73SC	113	HS Grade Level (F73) Source+	0-6
56	GL73F	114-115	HS Grade Level (F73) Flag*	0, 4, 8, 18, 28
57	GL73	116-117	HS Grade Level (F73) Code	-1 - Indeterminate 9 - 9th grade or less 10 - 10th grade 11 - 11th grade 12 - 12th grade

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
58	GL74SSC	118	HS Grade Level (S74) Source+	0-6
59	GL74SF	119-120	HS Grade Level (S74) Flag*	0, 4, 8, 10, 18
60	GL74S	121-122	HS Grade Level (S74) Code	-1 - Indeterminate 8 - 8th grade or less 9 - 9th grade 10 - 10th grade 11 - 11th grade 12 - 12th grade
61	GL74FSC	123	HS Grade Level (F74) Source+	0-6
62	GL74FF	124-125	HS Grade Level (F74) Flag*	0, 4
63	GL74F	126-127	HS Grade Level (F74) Code	-1 - Indeterminate 8 - 8th grade or less 9 - 9th grade 10 - 10th grade 11 - 11th grade 12 - 12th grade
64	HG73SC	128	Highest Grade Completed (F73) Source+	0-6
65	HG73F	129-130	Highest Grade Completed (F73) Flag*	0, 2, 4, 8, 12, 18
66	HG73	131-132	Highest Grade Completed (F73) Code	-1 - Indeterminate 8 - 8th grade or less 9 - 9th grade 10 - 10th grade 11 - 11th grade
67	HG74SSC	133	Highest Grade Completed (S74) Source+	0-6
68	HG74SF	134-135	Highest Grade Completed (S74) Flag*	0, 2, 4, 8, 12, 18
69	HG74S	136-137	Highest Grade Completed (S74) Code	-1 - Indeterminate 8 - 8th grade or less 9 - 9th grade 10 - 10th grade 11 - 11th grade 12 - 12th grade

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
70	HG74FSC	138	Highest Grade Completed (F74) Source+	0-6
71	HG74FF	139-140	Highest Grade Completed (F74) Flag*	0, 2, 4, 8, 18
72	HG74F	141-142	Highest Grade Completed (F74) Code	-1 - Indeterminate 8 - 8th grade or less 9 - 9th grade 10 - 10th grade 11 - 11th grade 12 - 12th grade
73	PSE73SC	143	PSE Status (F73) Source+	0-6
74	PSE73F	144-145	PSE Status (F73) Flag*	0, 4
75	PSE73	146-147	PSE Status (F73) Code	-1 - Indeterminate 1 - Currently in PSE 2 - Not in PSE, but previous exposure 3 - Current PSE status undetermined, but previous exposure 4 - PSE exposure undetermined, but currently in PSE 5 - Current PSE status undetermined, but no previous exposure 6 - Never in PSE
76	PSE74SSC	148	PSE Status (S74) Source+	0-6
77	PSE74SF	149-150	PSE Status (S74) Flag*	0, 2, 4, 8
78	PSE74S	151-152	PSE Status (S74) Code	-1 - Indeterminate 1 - Currently in PSE 2 - Not in PSE, but previous exposure 3 - Current PSE status undetermined, but previous exposure 4 - PSE exposure undetermined, but currently in PSE 5 - Current PSE status undetermined, but no previous exposure 6 - Never in PSE

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
79	PSE74FSC	153	PSE Status (F74) Source+	0-6
80	PSE74FF	154-155	PSE Status (F74) Flag*	0, 2, 4, 8, 10
81	PSE74F	156-157	PSE Status (F74) Code	-1 - Indeterminate 1 - Currently in PSE 2 - Not in PSE, but previous exposure 3 - Current PSE status undetermined, but previous exposure 4 - PSE exposure undetermined, but not currently in PSE 5 - Current PSE status undetermined, but no previous exposure 6 - Never in PSE
82	BGRADE	158-159	Best estimate of grade level in fall 1973 (contains missing data)	-8 - Not applicable -1 - Indeterminate 10 - 10th grade 11 - 11th grade 12 - 12th grade
83	UBI	160-161	UB Membership Index	-8 - Not applicable -1 - Indeterminate 0 - Never in UB 1 - Joined UB in 12th grade 2 - Joined UB in 11th grade 3 - In UB in 10th grade
84	UBPART	162-163	Grade by UB Membership Index	-8 - Not applicable -1 - Indeterminate 1 - 12th grader, in UB in 10th grade 2 - 12th grader, joined UB in 11th grade 3 - 12th grader, joined UB in 12th grade 4 - 12th grader, never in UB 5 - 11th grader, in UB in 10th grade 6 - 11th grader, joined UB in 11th grade 7 - 11th grader, never in UB 8 - 10th grader, in UB 9 - 10th grader, never in UB

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
85	BAL1	164	Grade Category for Balancing	0 - Not applicable 1 - 10th grade 2 - 11th grade 3 - 12th grade
86	BAL2	165-166	Class Category for Balancing	-8 - Not applicable 1 - Black, male, poor, risk 2 - Black, male, poor, not risk 3 - Black, male, not poor, risk 4 - Black, male, not poor, not risk 5 - Black, female, poor, risk 6 - Black, female, poor, not risk 7 - Black, female, not poor, risk 8 - Black, female, not poor, not risk 9 - White, poor, risk 10 - White, poor, not risk 11 - White, not poor, risk 12 - White, not poor, not risk 13 - Other, poor, risk 14 - Other, poor, not risk 15 - Other, not poor, risk 16 - Other, not poor, not risk
87	WE1	167	Followup Eligibility Flag	0 - Ineligible 1 - Eligible
88	W1	168-173	Raw Weight (WO) Adjusted for Followup Eligibility	(Implicit F6.2)
89	WE2	174	Eligibility Code for W2	0 - Ineligible 1 - Eligible
90	W2	175-181	W1 Adjusted for Telephone Subsampling	(Implicit F7.2)
91	WE3	182	Eligibility Code for Weights 3, 4, and 5	0 - Ineligible 1 - Eligible
92	W3	183-189	W2 Adjusted for Instrument Nonresponse	(Implicit F7.2)
93	W4	190-196	W3 Balanced Within Grade	(Implicit F7.2)

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
94	W5	197-203	W3 Balanced Over Grades	(Implicit F7.2)
95	WE6	204	Eligibility Code for Weights 6, 7, and 8	0 - Ineligible 1 - Eligible
96	W6	205-211	W3 with DUDS, DEADS Removed	(Implicit F7.2)
97	W7	212-218	W4 with DUDS, DEADS Removed	(Implicit F7.2)
98	W8	219-225	W5 with DUDS, DEADS Removed	(Implicit F7.2)
99	WE9	226	Eligibility Code for Weights 9 and 10	0 - Ineligible 1 - Eligible
100	W9	227-233	W7 Adjusted for UBPAR Indeterminacy	(Implicit F7.2)
101	W10	234-240	W8 Adjusted for UBPAR Indeterminacy	(Implicit F7.2)
102	WE11	241	Eligibility Code for Weights 11 and 12	0 - Ineligible 1 - Eligible
103	W11	242-248	W9 Adjusted for EDP Indeterminacy	(Implicit F7.2)
104	W12	249-255	W10 Adjusted for EDP Indeterminacy	(Implicit F7.2)
105	WE13	256	Eligibility Code for Weights 13 and 14	0 - Ineligible 1 - Eligible
106	W13	257-263	W9 Adjusted for HG|PE Indeterminacy	(Implicit F7.2)
107	W14	264- 270	W10 Adjusted for HG|PE Indeterminacy	(Implicit F7.2)
108	WE15	271	Eligibility Code for Weights 15 and 16	0 - Ineligible 1 - Eligible
109	W15	272-278	W9 Adjusted for EN|PE Indeterminacy	(Implicit F7.2)

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
110	W16	279-285	W10 Adjusted for EN|PE Indeterminacy	(Implicit F7.2)
111	WE17	286	Eligibility Code for Weight 17	0 - Ineligible 1 - Eligible
112	W17	287-293	W9 Adjusted for LENT Indeterminacy	(Implicit F7.2)
113	INUN	294	Institution Uniqueness	0 - Institution FICE already in file 1 - First or only appearance of FICE in file
114	ST0	295	Survey Eligibility Status	0 - Not eligible for survey 1 - Eligible for survey
115	ST1	296	First Mailout Status	0 - Not mailed 1 - Mailed
116	ST2	297	First Mailout Return Status	0 - Not returned 1 - Returned 2 - P.O. Returned as Undeliverable 3 - Deceased
117	ST3	298	Address Status	0 - 1974 Address 1 - Update available 2 - Update made
118	ST4	299	Second Mailout Status	0 - Not mailed 1 - Mailed
119	ST5	300	Second Mailout Return Status	0 - Not returned 1 - Returned 2 - P.O. Returned as Undeliverable 3 - Deceased
120	ST6	301	Scan Edit Status	0 - Passed edit 1 - Failed edit, not resolved 2 - Failed edit and resolved
121	ST7	302	Telephone Sample Selection Status	0 - Not selected 1 - Selected
122	ST8	303	Telephone Completion Status	0 - Not completed 1 - 100% completion 2 - Partial completion 3 - Deceased

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
123	ST9	304	Follow-Up Data Status	0 - Data not available 1 - Data entered: source Mail 1 2 - Data entered: source Mail 2 3 - Data entered: source Telephone 4 - Data entered: sources Merged Mail 1, Mail 2, or Telephone 5 - Data lost
124	FFU1	305-308	FU Question 1^{++} Highest Education Completed	1 - 10th grade 2 - 11th grade 3 - 12th Grade 4 - Less than 1 year voc/tech 5 - 1 year voc/tech 6 - 2 or more years voc/tech 7 - 1 year college/university 8 - 2 years college/university 9 - 3 years college/university 10 - 4 years college/university
125	FFU2	309-312	FU Question 2^{++} Currently Working	1 - No 2 - Yes, full-time 3 - Yes part-time
126	FFU3	313-316	FU Question 3^{++} Currently in Military	1 - Never been in 2 - In now 3 - Veteran
127	FFU4	317-320	FU Question 4^{++} Currently in School	1 - Not enrolled 2 - Enrolled part-time 3 - Enrolled full-time
128	FFU5	321-327	FU Question 5^{++} School Name	1001-12529 USOE FICE CODE 999 - High school 13020-Other PSE: Voc/Tech 13021-Other PSE: 2 yr./ 4 yr. colleges and universities
129	FFU6	328-331	FU Question 6^{++} Schooling Expected	1 - High school only 2 - Voc, trade, business school 3 - Some college 4 - 4-year college (B.A., B.S.) 5 - Graduate or professional degree
130	FFU7.1	332-335	FU Question 7.1^{++} Problems in Obtaining Education - Poor Grades	0 - Not circled 1 - Circled

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
131	FFU7.2	336-339	FU Question 7.2[++] Problems in Obtaining Education - Poor High School Preparation	0 - Not circled 1 - Circled
132	FFU7.3	340-343	FU Question 7.3[++] Problems in Obtaining Education - Personal/ Family Finances	0 - Not circled 1 - Circled
133	FFU7.4	344-347	FU Question 7.4[++] Problems in Obtaining Education - Inadequate Financial Aid	0 - Not circled 1 - Circled
134	FFU7.5	348-351	FU Question 7.5[++] Problems in Obtaining Education - Program Wanted Not Available	0 - Not circled 1 - Circled
135	FFU7.6	352-355	FU Question 7.6[++] Problems in Obtaining Education - Family Responsibilities	0 - Not circled 1 - Circled
136	FFU8.1	356-359	FU Question 8.1[++] Never Participated in UB	0 - Not circled 1 - Circled
137	FFU8.2	360-363	FU Question 8.2[++] Participated in UB in 10th grade	0 - Not circled 1 - Circled
138	FFU8.3	364-367	FU Question 8.3[++] Participated in UB in 11th grade	0 - Not circled 1 - Circled
139	FFU8.4	368-371	FU Question 8.4[++] Participated in UB in 12th grade	0 - Not circled 1 - Circled
140	EDP	372-374	Measure of Educational** Progress	10.0 - 16.0 (grade levels) F3.1
141	EDHS+	375-376	Persistance in Education Through or Beyond High School**	1 - Did not complete H.S. 2 - Completed at least H.S.

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
142	PENS	377-378	Persistance in Education Through PSE Entry**	1 - Never entered PSE 2 - Entered PSE
143	HG/PE	379-381	Persistance in PSE Given PSE Entry**	12.5 - 16.0 (grade levels) F3.1
144	EN/PE	382-383	1977 Enrollment in PSE Given Prior PSE Entry**	1 - Not currently enrolled 2 - Currently enrolled
145	LENT	384-385	Lag Entry into PSE**	1 - No lag entry 2 - Lag entry
146	WORK	386-387	1977 Work Status**	1 - Not working 2 - Working part-time 3 - Working full-time 4 - Working in military
147	MIL	388-389	1977 Military Status**	1 - Never in service 2 - Now in service 3 - Veteran
148	EDASP	390-391	Educational Aspirations**	1 - H.S. only 2 - Voc., trade, business school 3 - Some college 4 - 4 year college 5 - Grad. or professional degree
149	EDDET1	392-393	Were Poor Grades an Education Deterrent**	0 - No 1 - Yes
150	EDDET2	394-395	Was Poor H.S. Preparation an Educ. Deterrent**	0 - No 1 - Yes
151	EDDET3	396-397	Was Lack of Personal/ Family Finances an Education Deterrent**	0 - No 1 - Yes
152	EDDET4	398-399	Was Inadequate Financial Aid an Educ. Deterrent**	0 - No 1 - Yes
153	EDDET5	400-401	Was Lack of Desired Program an Education Deterrent**	0 - No 1 - Yes
154	EDDET6	402-403	Were Family Responsibilities an Education Deterrent**	0 - No 1 - Yes

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
155	AGE	404-405	Age as of Fall 1974**	14 or less 15 - 20 21 or older
156	RACE	406-407	Race**	1 - Black 2 - White 3 - Other
157	SEX	408-409	Sex**	1 - Male 2 - Female
158	BGRADE	410-411	Estimated Grade Fall 1973**	10 - 12
159	POVS	412-413	Poverty Status**	1 - Poor 2 - Not poor
160	RISK	414-415	Academic Risk Status**	1 - Risk 2 - Not risk
		416-417	Blank Field	
162	UBPAR	418-419	Extent of UB Participation in Years**	0 - 3
163	SNEP	420-421	Index of less-than-expected Educ. Progress	0 - Expected progress 1 - Less-than-expected progress
164	ISES	422-423	Institutional SES (Family Income Characteristics of Student Body)**	1 - Low SES 2 - Medium SES 3 - High SES
165	ISEL	424-425	Institutional Selectivity**	1 - Low selectivity 2 - Moderately low 3 - Moderately high 4 - High selectivity
166	ICOST	426-427	Cost of Attending Inst. Per Student**	1 - Less than $2,000 2 - $2,000-$2,499 3 - $2,500-$2,999 4 - $3,000-$3,999 5 - $4,000 or more

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
167	ISIZ	428-429	Size of Student Body** (FTE)	1 - Less than 499 2 - 500 - 999 3 - 1000-2999 4 - 3000-7999 5 - 8000 or more
168	IFAD	430-431	Percent of Student Body Receiving Federal Aid**	0 - None 1 - 1%-14% 2 - 15%-29% 3 - 30% or more
169	ISAID	432-433	Student Aid Dollars Per Student (FTE)**	0 - None 1 - $1 to $49 2 - $50 to $99 3 - $100 to $899 4 - $900 or more
170	IRACE	434-435	Predominant Race of Student Body**	1 - White 2 - Black
171	ISEX	436-437	Predominant Sex at Institution**	1 - Men 2 - Women 3 - Coed
172	ICONT	438-439	Type of Control**	1 - Public 2 - Private 3 - Proprietary
173	ITYPE	440-441	Type of Institution**	1 - 2-year or less 2 - 4-year, not university 3 - University
174	UBHOST	442-443	UB Host Status of Institution Attended**	0 - Not host 1 - Host
175	SSHOST	444-445	SSDS Host Status of Institution Attended**	0 - Not host 1 - Host
176	TSAM	446	Survey Return Status at Time of Telephone Sample Selection	0 - Mail respondent 2 - Nonrespondent 3 - Undeliverable
177	FALL	447	Availability of Fall 1974 Data	0 - Respondent, not applicable 1 - Data available 2 - Data not available

Variable Number	Mnemonic	Tape Position	Variable Description	Valid Codes
		448-449	Blank Field	
179	NEWDUD	450	Follow-up Data Utility Digit	0 - Data OK 1 - Originally sampled as UB now denies participation 2 - Originally sampled as CS now states UB participation
180	WE18	451	Eligibility Code for Weight 18	0 - Ineligible 1 - Eligible
181	W18	452-458	W9 Adjusted for SNEP Indeterminacy	(Implicit F7.2)
182	WE19	459	Weight eligibility for entire predictor set	0 - Ineligible 1 - Eligible
183	W19	460-466	W6 adjusted for indeterminacy of predictor variables and EDP	(implicit F7.2)
184	W20	467-473	W6 adjusted for indeterminacy of predictor variables and EDASP	(implicit F7.2)
185	W21	474-480	W6 adjusted for indeterminacy of predictor variables and PENS	(implicit F7.2)
186	CPROJ1	481-484	EDP Criterion-scaled project membership	(implicit F4.2)
187	CPROJ2	485-487	EDASP criterion-scaled project membership	(implicit F3.2)
188	CPROJ3	488-490	PENS criterion-scaled project membership	(implicit F3.2)

Footnotes

+ Source codes are as follows:

0 = No Data
1 = HSCR/PRV/SARF
2 = STF
3 = BSQ
4 = D/TQ
5 = FSQ
6 = Imputed

* Two-digit flag:

First digit:
0 = consistent across instruments
1, 2 = inconsistent across instruments

Second digit:
0 = original data
2 = logical imputation
4 = stochastic imputation
8 = inconsistency across variables

++ Follow-up data are coded in four or more digits. The field either contains a standard error code (see below) or a coded value. In all cases, the left-most byte or digit contains a consistency digit, the next is a quality digit, and the final digits are the datum itself. The diagram above illustrates the layout of a given field. The datum field could contain a standard error code or the actual datum. The quality digit can be a 0, 2, 4, or 8 (see below), and the consistency digit can be a 1 or 2 (see below).

Consistency Digit	Quality Digit	Datum
1 Digit	1 Digit	

Standard Error Codes

1) Missing data or nonresponses are coded -1.
2) Multiple responses are coded -2.
3) Legitimate nonresponses are coded -3.
4) Out-of-range or indeterminate responses are coded -4.
5) Don't know responses (when specific response category not present) are coded -5.

6) Nonresponses inconsistent with routing items are coded -6.

7) Refusals are coded -7.

8) Items not administered are coded -8.

9) Instrument nonresponse is coded -9.

Quality Digit Codes

All data fields include a quality digit.

1) Original data, not flagged, contain quality digit 0.

2) Data logically imputed contain quality digit 2.

3) Data stochastically imputed contain quality digit 4.

4) Original data not consistent with questionnaire skip patterns quality digit 6.

5) Original data logically inconsistent with other data items contain quality digit 8.

Consistency Digit Codes

0 = consistent over instruments

1 = inconsistent over instruments

** Analysis variables (variables 140-175) contain standard codes to indicate various common states for missing or omitted data; these are:

-1: Missing or indeterminate data

-3: Legitimate nonresponse

-8: Item not applicable

-9: Instrument nonresponse

Appendix E

Sample Weight Adjustments

Appendix E

Sample Weight Adjustments

I. OVERVIEW

Two conceptually different weight adjustment procedures are discussed in this appendix, both of which were employed to provide more meaningful analyses. The first procedure discussed below is a technique whereby an artificial comparison population was constructed to be directly comparable to the UB participant population on relevant student background variables (sex, race, poverty status, and academic risk status) and on initial grade cohort distribution. In essence, the procedure forces an equivalence of the two groups in terms of characteristics that are theoretically related to outcomes but that were not completely controlled by the study design. This adjustment procedure (sometimes called standardization or balancing) was used only for the comparative descriptive analyses reported in Chapter 4.[1]

The second procedure discussed in this appendix involves the adjustment of sample weights for the subsampling procedures used, for instrument nonresponse, and for item indeterminancy. The need for weight adjustment due to subsampling is rather intuitive and obvious. The adjustment for nonresponse or item indeterminancy is a method used to correct results for possible bias introduced by the failure to contact some individuals or for the provision of incomplete data by some individuals. Such adjustments involved an apportioning of sampling weights of nonrespondents among the respondents who were most like them. These adjustment procedures were used for all the major analyses reported in Chapters 4 and 5.

Discussion of the actual procedures used and implications for estimation are given in subsequent sections of this appendix. The remainder of this introductory section will present certain considerations basic to the use of the techniques.

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[1] Since background variables and initial grade were parameters in the regression models used in analyses reported in Chapter 5, the use of this technique was not required for the relational analyses.

A. Considerations of Corrections for Group Differences

In cases such as the current one, where moderating variables (individual background variables and initial grade cohort) are related to criterion variables (educational outcomes) and where the moderating variables are differentially distributed within the groups under consideration (former participants and nonparticipants), it is usually sound analytic practice to adjust or correct the criterion variables for the influence of the moderating variables. There are many techniques available to accomplish this adjustment process. Perhaps the most widely used is linear regression, in which the linear relation of moderating variable to criterion variable is removed (e.g., analysis of covariance, partial correlation techniques). A very simple form of this technique is well adapted to quantitative data when one is willing to assume that the majority of the moderator's influence is linear and equivalent for different subgroups.

Another approach to data adjustment is the analysis of variance model. This technique, considered within the framework of general linear models, may be used to examine the "effects" (both main effects and interaction effects) of specific categorical factors after having removed any effect attributed to other categorical factors. Like the linear regression model, relatively strong assumptions about the data must be made (e.g., equality of variances over subgroups), and this technique is best applied to quantitative criterion variable data. It would be difficult to employ a complete analysis of variance model, since the number of cases in some cells of the model would be extremely small. Further, since much of the criterion variable data was stated in proportions, considerable data transformation (i.e., log or arc sine transformations) might be required, and reporting such transformed data often clouds interpretation.

Since assumptions of the general linear model were made in conducting the relational analyses reported in Chapter 5, additional reliance on these assumptions for the comparative descriptive analyses did not seem warranted. Instead, the comparative descriptive analyses were based on procedures that are similar to those used by demographers in constructing "adjusted," "standardized," or "corrected" birth and death rates.[2]

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[2] See Spiegelman, M. Introduction to demography. Cambridge, Massachusetts: Harvard University Press, 1968.

B. Considerations of Adjustments for Instrument and Item Nonresponse

For the complex sample design used in the UB evaluation, individuals were selected into the sample with unequal probabilities. Therefore, to inflate the sample to the size of the population being estimated (so that each person represented the correct number of persons in the population), sample weights that were inversely proportional to the probability of selection were applied to individual responses. (See Appendix A for a discussion of the use of sample weights.) In producing these national estimates, some type of adjustment was needed to correct for the failure of individuals to return questionnaires or to return fully completed questionnaires.

The two most common techniques for handling indeterminate data are imputation techniques and weight adjustment techniques. There are many specific imputation techniques available, but all of them involve creating data for those individuals who have not responded to a particular item. Some techniques assign the mean value of similar individuals; others assign the values of the one individual most like the nonrespondent; still others involve simple or iterative regression models to predict (according to specified criteria) what the missing data would most likely be. Similarly, there are various available weight adjustment procedures; however, they all involve assignment of the weights of nonrespondents to those who responded to a particular instrument or item.

In general, the method of weight adjustment used in this study involved the proportional distribution of a nonrespondent's sampling weight to that group of responding individuals judged to be most similar to him. In terms of estimates of means and proportions obtained, this technique yields results that are equivalent to assigning the nonrespondent an imputed value of the group mean of similar respondents. In terms of standard error computation, the two techniques do not yield equivalent results; larger standard errors are obtained by the weight adjustment method. RTI chose the more conservative weight adjustment procedure.

II. INTRODUCTION TO THE BALANCING PROBLEM

The comparison population (i.e., the group against which former UB participants were to be compared) was defined as all nonparticipants enrolled in the tenth, eleventh, or twelfth grade at high schools served by a UB project in September 1973. There was no reason to believe, however, that the distributions of this population on relevant background characteristics would be exactly the same as those of the UB participant population due to UB selection criteria. As shown in Table F.40, Appendix F, the groups differed substantially on certain background variables. Therefore, any direct comparisons between UB participants and nonparticipants with respect to educational continuance measures would not take into account the fact that the groups being compared would be different on characteristics such as family income, ethnic background, etc. that are related to educational continuance.

It is possible, however, to make an adjustment for such differences in these background characteristics by using a fairly common procedure of demography. The demographer, for example, computes age-specific birth rates for each relevant age group and applies them to a population of interest, age group by age group, in order to obtain an "age adjusted" birth rate. In a similar fashion one could, for example, compute current enrollment rates for each of several family income categories, for former UB participants and also for nonparticipants. These rates could then be applied to a population of individuals having a certain income distribution, and adjusted enrollment rates could then be computed for UB participants and also for the comparison group.

The comparison of these two adjusted retention rates would then be free of the effects of differential family income distributions among former UB participants and nonparticipants. By computing such adjusted rates, the effects of differences in background characteristics of the UB participants and the comparison group could be reduced or eliminated. The population used as a standard could be either of the two populations being compared. For example, instead of standardizing the rates to an outside population, the rates of the comparison group could be standardized to the distribution of the UB group, which was the approach used in this study.

A hypothetical example is presented here to show how different background characteristics could affect the results of comparing two groups, such as UB participants and the comparison group, and how a balancing or standardization procedure could be employed. Suppose that a situation existed as shown in Table E.1.[3] From the table it can be seen that the two groups (A and B) differ in terms of the proportion of students falling into the levels of poverty status (80 percent of those in group A are classified as within the poverty level, while only 20 percent of those in the unbalanced group B are so classified). Moreover, it is seen that within both groups, poverty status classification is related to average grade point average (GPA), i.e., lower grade point

Table E.1

EXAMPLE OF BALANCING TECHNIQUE USING HYPOTHETICAL DATA
RELATING TO GRADE POINT AVERAGE AND POVERTY STATUS

	Group Classification					
	Group A		Group B Unbalanced		Group B Balanced	
Poverty Status Classification	Proportion of Students	Average GPA	Proportion of Students	Average GPA	Proportion of Students	Average GPA
Poverty Level	.80	2.0	.20	1.5	.80	1.5
Not Poverty Level	.20	3.0	.80	2.5	.20	2.5
Total Group[a]	1.00	2.2	1.00	2.3	1.00	1.7

NOTE: These data are purely hypothetical and do not reflect the true distributions for any group on poverty status or on any measure of GPA used in this study.

[a] The average GPA for any group is obtained as a weighted sum of the subgroup means within that group. For example, the total group A average GPA is determined as (.80)(2.0) + (.20)(3.0) = 2.2.

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[3] The problem posed in the example may be recognized as equivalent to the nonorthogonality problem in analysis of variance.

averages are associated with poverty level classification. More importantly, it can be seen that within either level of poverty status classification, group A has a higher average GPA than group B (by .5).

To this point, all indications from the hypothetical data in Table E.1 would indicate that those in group A were obtaining better grades than those in group B, regardless of poverty status classification. Some confusion arises, however, when comparing the total group means for group A and the unbalanced group B. The total group mean is obtained as a weighted average of the subgroup (poverty level classification) means within that group. The group A average GPA is given by (.80)(2.0) + (.20)(3.0) = 2.2, while for the unbalanced group B it is (.20)(1.5) + (.80)(2.5) = 2.3. The results of this averaging indicate that group B has, on the average, a higher grade point average than group A. This is a statistical fact; even though the subgroup means stand in an order clearly favoring group A, the total group means are juxtaposed. The apparent anomaly has been brought about due to the differential distribution of the two groups with respect to poverty status, coupled with the fact that poverty status itself is related to GPA.

If the analytic question to be answered is concerned only with group means, irrespective of any difference which may exist between groups on the distribution of the moderating variable, then the answer is clearly that group B has a higher grade point average than group A. If, on the other hand, the analytic question is concerned with differences between the two groups free of the effect of differential poverty status, then it is equally clear that a comparison of the uncorrected group means obscures the true situation. The balancing technique used in this study would attempt to resolve the incongruity by creating an artificial group B population which is comparable to group A in terms of the poverty status distribution (i.e., a group B in which 80 percent of the members were classified as poverty level). This is the situation shown for the balanced group B in Table E.1. Comparing the total group A mean for GPA (2.2) to the balanced group B mean (1.7) reflects the fact that members of group A, within any poverty classification, have an average GPA which is greater by .5 than that for members of group B. In other words, when adjustment is made for differential distribution of the moderating variable within the two groups, the effect of that variable is balanced across the two groups.

While the actual balancing technique used in this study involved more than two balancing categories, the example given shows the general approach used. The technique statistically equates the joint distribution of possible moderating variables within the two groups (e.g., it forces the proportion of poor, academic risk, black, female, twelfth-grade students in the comparison group to be identical to the analogous proportion in the UB group, etc.). This technique makes fewer assumptions of the data than either the analysis of variance or regression techniques and allows somewhat greater flexibility in the choice of data elements to be adjusted. Obviously, this method can be applied for one, two, or more moderating variables, either jointly or singly, provided cell frequencies are of sufficient size to provide stable estimates of all proportions and means.

As a result of examinations of group differences (see Appendix F, Table F.40), the following variables were used in balancing (adjustment): sex, race, grade level, poverty status, and academic risk status. Since many of the critical analyses were conducted within initial grade cohort, one adjustment was performed within the three grade cohorts considered. Another adjustment over grade cohort, which also accounted for differential initial grade distributions, was carried out for aggregate comparisons.

III. SPECIFIC PROCEDURES USED IN BALANCING

The procedures used in balancing involved standardizing the characteristics of the comparison group to the UB population characteristics within each of the three initial grade cohorts 10-12 as well as over grade. For each of grades 10, 11, and 12, 16 balancing groups were formed both for UB participants and for the comparison group. The 16 balancing groups were based on two variables associated with UB participation qualifications, namely poverty status and academic risk, and on two additional variables, ethnicity and sex. The 16 balancing groups were formed as a partial crossing of these four variables as indicated in Table E.2.

Within each of the three grades separately and for each of the 16 balancing categories, the total estimated relative population size was obtained for UB students and for comparison students. Using these numbers, two balancing weight components were computed for each category (one for within-grade adjustment and one for over-grade adjustment). Each comparison student's final sample weight was then multiplied by each balancing weight component appropriate to his categorization to obtain his two final balanced weights. Balanced or standardized rates, proportions, etc. could then be estimated for the comparison students (whether within a given grade or in aggregate) by using the appropriate final balanced weights rather than the original sample weights when applying the estimation formulas presented in Appendix A. Comparing UB participant statistics with balanced comparison statistics would then be free of the effects of differential distributions among the balancing groups.

Table E.2

DESCRIPTION OF THE SIXTEEN CATEGORIES USED IN BALANCING WITHIN GRADE

	Poverty Status and Academic Risk			
Ethnicity and Sex	Poverty Risk	Poverty "Not Risk"	"Not Poverty" Risk	"Not Poverty" "Not Risk"
Black, male	1	2	3	4
Black, "not male"	5	6	7	8
White, all sexes	9	10	11	12
Other, all sexes	13	14	15	16

NOTE: Table entries indicate the 16 balancing group numbers as a function of the marginal classifiers. The categories "not male," "not risk," and "not poverty," include cases which could not be classified due to indeterminate data as well as cases that could be classified as female, not risk, and not poverty, respectively.

The computations for within-grade and over-grade balancing are given in the following subsections.[4]

A. Balancing Within Grade

For a given grade g, g ← 10, 11, 12, define the estimated number of UB students in grade g and in balancing category k, k ← 1, 2, ..., 16, as $Y_{ug}(k)$, which was determined as follows:

$$Y_{ug}(k) = \sum_{j=1}^{N} W_j U_j G_j K_j E_j ;$$

where

N is the total number of individuals (N=6050),

W_j is the eligible-adjusted weight for individual j,

U_j is a UB status indicator, with value 1 if individual j indicated UB participation and 0 otherwise,

G_j is a grade status indicator, with value 1 if initial grade cohort of individual j was g and 0 otherwise,

K_j is a group status indicator, with value 1 if the balancing group of individual j was k and 0 otherwise,

E_j is an eligibility indicator, with value 1 if individual j was eligible for the survey and 0 otherwise.

Also define, for grade g, the estimated number of nonparticipants in grade g and balancing category k as $Y_{cg}(k)$, which was determined as follows:

$$Y_{cg}(k) = \sum_{j=1}^{N} W_j C_j G_j K_j E_j ,$$

---

[4] Note that the notation used in presenting formulas in this appendix is somewhat different from that used in Appendix A. In general, subscripts and superscripts are used consistently as follows:

j, indicates an individual;
k ← 1, 2, ..., 16, indicates a balancing category;
h ← 1, 2, ..., 27, indicates project stratum;
i ← 1, 2, indicates project within stratum;
g ← 10, 11, 12, indicates initial grade cohort;
u, indicates former Upward Bound participant;
c, indicates nonparticipant; and
' is used to indicate a balanced value.

where C_j is $1-U_j$ and where N, W_j, G_j, K_j, and E_j are defined above.

Now $\pi_{ug}(k)$, the proportion of the grade g UB population in balancing class k, was defined as

$$\pi_{ug}(k) = \frac{Y_{ug}(k)}{\sum_{k=1}^{16} Y_{ug}(k)} .$$

An analogous proportion, $\pi_{cg}(k)$ was also determined for the comparison group as

$$\pi_{cg}(k) = \frac{Y_{cg}(k)}{\sum_{k=1}^{16} Y_{cg}(k)} .$$

Define the balancing weight correction component for category k as $\omega_g(k)$, where

$$\omega_g(k) = \frac{\pi_{ug}(k)}{\pi_{cg}(k)} .$$

The within-grade balanced weight for an individual, $\beta_g(j)$, was then determined within grade g and balancing category as follows:[5]

$$\beta_g(j) = \begin{cases} W_j \, E_j, \text{ if } C_j = 0, \\ W_j \, E_j \, \omega_g(k), \text{ if } C_j = 1. \end{cases}$$

B. Balancing Over Grade

Using the procedures outlined above for determining $Y_{ug}(k)$ and $Y_{cg}(k)$, the proportions of UB students in grade g and balancing category k, $\pi_u(g,k)$, and the analogous proportion for comparison students, $\pi_c(g,k)$, were determined as follows:

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[5] Note that the weight of UB participants (C_j=0) is not affected by the balancing. The use of the value E_j in this process is somewhat redundant, since it only serves to set the weight of ineligibles to 0. This had already been accomplished at the time balancing operations were implemented, with any appropriate adjustments (see Section VI of this appendix).

$$\pi_u(g,k) = \frac{Y_{ug}(k)}{\sum_{g=10}^{12} \sum_{k=1}^{16} Y_{ug}(k)} ,$$

and

$$\pi_c(g,k) = \frac{Y_{cg}(k)}{\sum_{g=10}^{12} \sum_{k=1}^{16} Y_{cg}(k)} .$$

The balancing weight correction component for grade g and class k, $\omega(g,k)$, is given by

$$\omega(g,k) = \frac{\pi_u(g,k)}{\pi_c(g,k)} .$$

The over-grade balanced weights for an individual, $\beta_.(j)$, within grade g and balancing class k was then determined as follows:

$$\beta_.(j) = \begin{cases} W_j\ E_j, \text{ if } C_j = 0 , \\ W_j\ E_j\ \omega(g,k), \text{ if } C_j = 1 . \end{cases}$$

IV. BALANCED ESTIMATES AND THEIR STANDARD ERRORS[6]

A. Balanced Means or Proportions for the Comparison Group

A balanced estimated mean or proportion for the comparison group within grade g was obtained as follows:

$$P'_{cg} = \sum_{h=1}^{27} \sum_{i=1}^{2} X'_{cg}(hi) \Big/ \sum_{h=1}^{27} \sum_{i=1}^{2} Y'_{cg}(hi) \qquad \text{(E.1)}$$

$$= X'_{cg}/Y'_{cg} ,$$

---

[6] The equations given in this section are for within-grade computations using the balanced weights $\beta_g(j)$. Computations for aggregate statistics, using weights $\beta_.(j)$ are analogous and thus are not presented.

where

$$X'_{cg}(hi) = \sum_{j=1}^{n_c(hi)} \beta_{cg}(hij)Y_{cg}(hij)X_{cg}(hij) \ ,$$

and

$$Y'_{cg}(hi) = \sum_{j=1}^{n_c(hi)} \beta_{cg}(hij)Y_{cg}(hij) \ ,$$

represent the weighted and balanced comparison group totals for variables X and Y associated with the project hi. With the Y variable indexing a specific subpopulation or domain, the X and Y variables are:

$X_{cg}(hij)$ = a response variable for grade g comparison student j from project i in stratum h,

$$Y_{cg}(hij) = \begin{cases} 1 & \text{if grade g comparison student hij belongs to the domain of interest,} \\ 0 & \text{otherwise,} \end{cases}$$

$\beta_{cg}(hij)$ = the within-grade adjusted or "balanced" weight for grade g comparison group member hij, and

$n_c(hi)$ = the number of comparison students in project hi.

B. Sampling Error (Standard Error) of Balanced Means or Proportions for the Comparison Group

To approximate the variance of P'_{cg}, the following Taylorized deviation is formed following the general theory advanced by Woodruff.[7]

$$Z'_{cg}(hi) = [R\{X'_{cg}(hi)\} - R\{Y'_{cg}(hi)\}] \qquad \text{(E.2)}$$

$$+ \sum_{k=1}^{K} \pi'_{cg}(k) \ \frac{P'_{cg}(k)-P'_{cg}}{P'_{cg}} \ [R\{\eta_{ugk}(hi)\} - R\{\eta'_{cgk}(hi)\}] \ ,$$

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[7] Woodruff, R. S. Simple method for approximating variance of a complicated estimate. Journal of the American Statistical Association, June 1971, Vol. 66, Number 334, pp. 411-414.

using the X and Y sums, by project, from (E.1) to obtain the ratios[8]

$$R\{X'_{cg}(hi)\} = X'_{cg}(hi)/X'_{cg},$$

and

$$R\{Y'_{cg}(hi)\} = Y'_{cg}(hi)/Y'_{cg} \quad .$$

Notice that these ratios represent fractions of the balanced comparison group totals in X and Y contributed by the specific projects. Also, recalling the Taylorized deviation for unbalanced totals, one observes that the bracketed first term in $Z'_{cg}(hi)$ above is the standard linearization for the rel-variance of a ratio. The remaining term in $Z'_{cg}(hi)$ accounts for the sampling variation induced by balancing. This second term is a weighted sum over the K balancing groups with the weights based on

$$P'_{cg}(k) = \sum_{h=1}^{27} \sum_{i=1}^{2} X'_{cgk}(hi) / \sum_{h=1}^{24} \sum_{i=1}^{2} Y'_{cgk}(hi) \qquad \text{(E.3)}$$

$$= X'_{cg}(k)/Y'_{cg}(k),$$

the comparison group proportion for balancing cell k, where

$$X'_{cgk}(hi) = \sum_{j=1}^{n_c(hi)} K_{cgk}(hij)\beta_{cg}(hij)Y_{cg}(hij)X_{cg}(hij) \ ,$$

and

$$Y'_{cgk}(hi) = \sum_{j=1}^{n_c(hi)} K_{cgk}(hij)\beta_{cg}(hij)Y_{cg}(hij) \ ,$$

with the K variable indicating specific balancing cells, that is,

$$K_{cgk}(hij) = \begin{cases} 1 & \text{if grade g comparison student hij belongs} \\ & \text{to balancing cell k,} \\ 0 & \text{otherwise.} \end{cases}$$

---

[8] In general, throughout this presentation, the notation R{·} is used to indicate a ratio.

Using the balanced domain total for the comparison group from balancing cell k, namely $Y'_{cg}(k)$ from (E.3), and the corresponding domain total across all K cells, or Y'_{cg} from (E.1), the quantity $\pi'_{cg}(k)$ in (E.2) represents the following cell fractions:

$$\pi'_{cg}(k) = Y'_{cg}(k)/Y'_{cg} \ . \qquad \text{(E.4)}$$

Notice that

$$\sum_{k=1}^{K} \pi'_{cg}(k) = 1 \ ,$$

and

$$\sum_{k=1}^{K} \pi'_{cg}(k)\ P'_{cg}(k) = P'_{cg} \ .$$

Finally, the last bracketed term within the summation involves the balanced comparison group population totals, where

$$N'_{cg}(k) = \sum_{h=1}^{27} \sum_{i=1}^{2} \eta'_{cgk}(hi)$$

for balancing cell k, with the separate project contributions formed as

$$\eta'_{cgk}(hi) = \sum_{j=1}^{n_c(hi)} K_{cgk}(hij)\beta_{cg}(hij).$$

Similarly, for the Upward Bound sample one sums project contributions

$$\eta_{ugk}(hi) = \sum_{j=1}^{n_u(hi)} K_{ugk}(hij)\beta_{ug}(hij) \ ,$$

where terms differ only in the use of the subscript u and the absence of the prime notation,[9] to estimate the Upward Bound population total

$$N_{ug}(k) = \sum_{h=1}^{27} \sum_{i=1}^{2} \eta_{ugk}(hi) \quad .$$

With these definitions, the differences between comparison group and Upward Bound fractions,

$$R\{\eta'_{cgk}(hi)\} = \eta'_{cgk}(hi)/N'_{cg}(k), \tag{E.5}$$

and

$$R\{\eta_{ugk}(hi)\} = \eta_{ugk}(hi)/N_{ug}(k), \tag{E.6}$$

are seen to account for the relative variance of balancing ratios

$$N_{ug}(k)/N'_{cg}(k) \quad .$$

The variance of a balanced mean or proportion for comparison students is therefore given by

$$VAR(P'_{cg}) = (P'_{cg})^2 \sum_{h=1}^{27} [Z'_{cg}(h1)-Z'_{cg}(h2)]^2 \ , \tag{E.7}$$

and the associated standard error is

$$SE\ (P'_{cg}) = \sqrt{VAR\ (P'_{cg})} \quad . \tag{E.8}$$

C. Means or Proportions for Upward Bound Students

Because the comparison student subpopulations were balanced to the characteristics of the Upward Bound subpopulations, no balancing is involved when estimating a mean or proportion for UB students. The estimate would be obtained as the ratio of two Upward Bound totals; namely,

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[9] Since the balancing procedure did not affect the sampling weights for Upward Bound participants, the prime (') notation is not used even though the weight $\beta_{ug}(hij)$ appears in the equations.

$$P_{ug} = \sum_{h=1}^{27} \sum_{i=1}^{2} X_{ug}(hi) / \sum_{h=1}^{27} \sum_{i=1}^{2} Y_{ug}(hi) \qquad \text{(E.9)}$$

$$= X_{ug}/Y_{ug} \quad ,$$

with the project contributions formed as

$$X_{ug}(hi) = \sum_{j=1}^{n_u(hi)} \beta_{ug}(hij)Y_{ug}(hij)X_{ug}(hij),$$

and

$$Y_{ug}(hi) = \sum_{j=1}^{n_u(hi)} \beta_{ug}(hij)Y_{ug}(hij) \ .$$

If the ratio P_{ug} corresponds to a subgroup mean for response variable X, then

$X_{ug}(hij)$ = a response variable for grade g UB student j from project i in stratum h,

$$Y_{ug}(hij) = \begin{cases} 1 & \text{if grade g UB student hij belongs to the domain of interest,} \\ 0 & \text{otherwise,} \end{cases}$$

and

$\beta_{ug}(hij)$ = the sample weight for UB student hij.

The appropriate linearized value for the relative variance of P_{ug} is simply

$$Z_{ug}(hi) = [R\{X_{ug}(hi)\} - R\{Y_{ug}(hi)\}] \ , \qquad \text{(E.10)}$$

where

$$R\{X_{ug}(hi)\} = X_{ug}(hi)/X_{ug} \quad ,$$

and

$$R\{Y_{ug}(hi)\} = Y_{ug}(hi)/Y_{ug} \quad .$$

The corresponding variance estimator is

$$\text{VAR }(P_{ug}) = (P_{ug})^2 \sum_{h=1}^{27} [Z_{ug}(h1)-Z_{ug}(h2)]^2 \quad , \qquad \text{(E.11)}$$

and

$$\text{SE }(P_{ug}) = \sqrt{\text{VAR }(P_{ug})} \quad . \qquad \text{(E.12)}$$

D. Single Grade Upward Bound vs. Balanced Comparison Student Contrasts

Let

$$\delta'(g) = P_{ug}-P'_{cg} \qquad \text{(E.13)}$$

denote a balanced grade g difference between corresponding Upward Bound and Comparison Means (or proportions). The associated linearization value for the variance of δ'(g) is obtained by combining the linearizations developed in equations (E.2) and (E.10), namely

$$Z'_g(hi) = [P_{ug}Z_{ug}(hi) - P'_{cg}Z'_{cg}(hi)] \quad , \qquad \text{(E.14)}$$

and, therefore,

$$\text{VAR }(\delta'(g)) = \sum_{h=1}^{27} [Z'_g(h1)-Z'_g(h2)]^2 \quad , \qquad \text{(E.15)}$$

and

$$\text{SE }(\delta'(g)) = \sqrt{\text{VAR }(\delta'(g))} \quad . \qquad \text{(E.16)}$$

V. WEIGHT ADJUSTMENTS FOR SUBSAMPLING AND NONRESPONSE[10]

The weight adjustment techniques described in this section are those that were implemented to compensate for the fact that data were not collected for all 6,050 individuals in the original sample. The weight adjustments discussed here involve: (a) corrections for telephone subsampling and (b) corrections for indeterminate response.[11] The various weight adjustment techniques (including balancing) were applied sequentially, and the order in which they were applied was important in determining an individual's final weight. The order of weight adjustments is presented in Section VI of this appendix.

Clearly, some adjustment in weights was required due to subsampling of mail nonrespondents, since the telephone responses of those selected were used to represent not only themselves but also those mail nonrespondents who were not selected. Since the sampling within stratum was random, the subsampling adjustment procedure involved multiplying selected nonrespondents' weights by the inverse of the appropriate within-stratum sampling rate (see Appendix A), and setting the weights of nonselected nonrespondents to zero.[12]

The correction for instrument and item nonresponse (or indeterminancy) is not as immediately obvious but is equally important. It is reasonable to expect that the aggregate values of responses of individuals for whom data were not collected would have been different from the aggregate values of those for whom data were collected (i.e., nonresponse is not considered to be a random process, and, therefore, is not independent of the study variable set). For example, the following assumptions are not unreasonable. The more mobile an individual, the less likely he would be located for data collection purposes and the less likely he would be expected to have obtained higher levels of educational progress. Likewise, the less satisfied an individual is with his educational progress, the less likely he would be willing to report it.

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[10] The notation used in this section is similar to that of previous sections of this appendix (see footnote 4, page E.9) with the following exceptions: (1) the term $W_{\bullet}(\cdot)$ is used to denote the weight at the start of any step of the sequential adjustment procedure, and the term $W'_{\bullet}(\cdot)$ is used to denote the weight at the conclusion of that step (as such, the terms $W_{\bullet}(\cdot)$ and $W'_{\bullet}(\cdot)$ may represent balanced or unbalanced weights depending upon the stage being considered--see Section VI of this appendix for the order of balancing), and (2) the notation ℓ is used to indicate the strata used in selecting the subsample of mail nonrespondents.

[11] An additional form of weight correction was employed involving the exclusion from analyses of individuals determined survey-ineligible, specifically setting the weights of such individuals to 0. This correction is straightforward and is not described in this section; however, it is referenced in Section VI of this appendix.

[12] The expected value of the adjusted weight totals over all possible subsamples is the unadjusted weight total, but, for a particular subsample, equivalence of adjusted and unadjusted weight totals is neither guaranteed nor expected.

For cases in which nonrespondents and respondents are expected to differ, spurious results could obtain if one were simply to ignore nonrespondents or to assign their weights equally to all respondents. The procedure used involved proportionally assigning the weights of nonrespondents to those respondents who were most like them. For purposes of this study, "most like" was defined as from the same project, in the same grade, and in the same balancing category (i.e., of the same sex, race, poverty status, and academic risk status).[13]

A. Specific Procedures Used in Adjusting for Subsampling

This adjustment was performed exclusively for the subset of individuals who were mail nonrespondents or undeliverables at the time of telephone sampling. Since sampling rates were differential among the four strata used for the subsampling (see Appendix A), the adjustments were made within those strata.

Within each stratum, ℓ, $\ell \leftarrow 1, 2, 3, 4$, the stratum sampling rate, R_ℓ was determined as

$$R_\ell = \frac{A_\ell}{A_\ell + B_\ell}$$

where

A_ℓ = the number of selected mail nonrespondents in stratum ℓ,

and

---

[13] The unadjusted and adjusted weight totals will be identical, within rounding error, under this adjustment procedure.

B_ℓ = the number of nonselected mail nonrespondents in stratum ℓ.

The weight adjustment factor for stratum ℓ, ω_ℓ, was computed as

$$\omega_\ell = \frac{1}{R_\ell} ,$$

and the telephone sample selection adjusted weight for individual j in stratum ℓ, $W'_{\ell j}$, was defined as

$$W'_{\ell j} = \omega_\ell W_{\ell j} S_{\ell j}$$

where

$W_{\ell j}$ = the unadjusted[14] weight for individual j in stratum ℓ,

and

$$S_{\ell j} = \begin{cases} 1 & \text{if individual j was a selected mail nonrespondent in stratum } \ell, \\ 0 & \text{if individual j was a nonselected mail nonrespondent in stratum } \ell. \end{cases}$$

Values of ω_ℓ for each of the four strata as defined in Appendix A[15] were 1.133, 2.880, 5.915, and 9.050, respectively.

B. Specific Procedures Used in Adjusting for Nonresponse or Indeterminancy

In cases of nonresponse or other forms of indeterminate data, weight adjustments were made. In general, such adjustments involved apportioning the sampling weights of nonrespondents proportionally among the respondents who were in a category most like them (e.g., to those responding individuals from the same project or set of schools and of the same sex, race, academic risk status, poverty status, and grade level). The equations given are general and apply to any given adjustment of this type.[16] All such adjustments were performed separately for the former UB participant and nonparticipant groups, but for purposes of parsimony the equations given below are for the former participant group only (completely analogous equations would apply for the nonparticipant group).

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[14] This is not the raw sampling weight for the individual, but reflects other weight adjustments made to raw weights prior to the specific adjustment for telephone sample selection.

[15] The four strata were: (1) "nonrespondents" with available fall 1974 data, (2) "nonrespondents" without available fall 1974 data, (3) "undeliverables" with available fall 1974 data, and (4) "undeliverables" without available fall 1974 data.

Define the unadjusted[17] weight totals of UB respondents and nonrespondents in grade cohort g and balancing category k of project hi as $T^r_{ugk}(hi)$ and $T^n_{ugk}(hi)$, respectively. These totals are given by

$$T^r_{ugk}(hi) = \sum_{j=1}^{n_{ugk}(hi)} R_{ugk}(hij)\ W_{ugk}(hij),$$

and

$$T^n_{ugk}(hi) = \sum_{j=1}^{n_{ugk}(hi)} [1-R_{ugk}(hij)]W_{ugk}(hij)\ ,$$

where

$W_{ugk}(hij)$ = the unadjusted weight of former UB participant j in project hi grade cohort g and balancing category k,

$$R_{ugk}(hij) = \begin{cases} 1 & \text{if former participant j in project hi grade cohort g and balancing category k was a respondent,} \\ 0 & \text{otherwise,} \end{cases}$$

and

$n_{ugk}(hi)$ = the number of former participants in project hi grade g and balancing category k.

The weight of former participant nonrespondents was then to be apportioned to former participant respondents--within project hi, grade g, and balancing category k--proportional to the respondents' unadjusted weights to give the adjusted weight, $W'_{ugk}(hij)$, for individual j. The adjusted weight was therefore defined as

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[16] Several such adjustments were performed as described in Section VI of this appendix.

[17] Unadjusted in respect to the adjustment under consideration.

$$W'_{ugk}(hij) = R_{ugk}(hij)\ W_{ugk}(hij)\ \ 1 + \frac{T^{n}_{ugk}(hi)}{T^{r}_{ugk}(hi)} \ .$$

Note that the adjustment is not defined for the case in which there are nonrespondents but no respondents in project hi, grade g, and classification k; however, when the respondent group exists,

$$\sum_{j=1}^{n_{ugk}(hi)} W'_{ugk}(hij) = T^{r}_{ugk}(hi) + T^{n}_{ugk}(hi) = \sum_{j=1}^{n_{ugk}(hi)} W_{ugk}(hij).$$

Thus, for the case in which the procedure was undefined, it was necessary to change the definition of the group "most like" the nonrespondents. This process of redefinition was approached systematically.

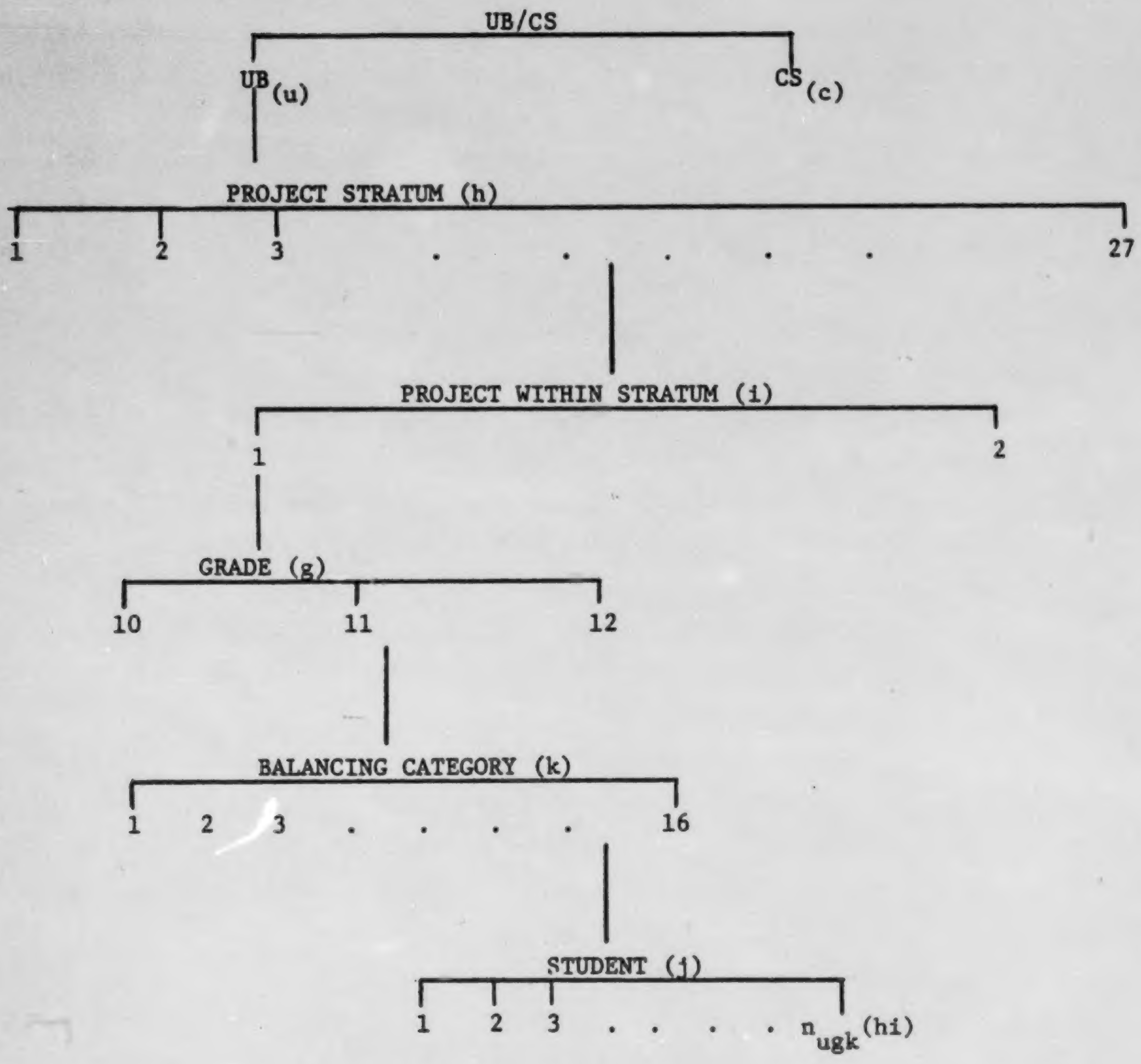
Figure E.1 depicts a hierarchical structure of categories for study participants. The weight adjustment software involved defining 2,592 categories of students (27 x 2 x 3 x 16) within the comparison and UB groups. There were many empty cells and, on occasion, cells with no respondents, only nonrespondents. In these cases, the category definitions were collapsed to the next highest level in the hierarchy, and the weight adjustment performed in a manner completely analogous to that defined above. That is, if in a given balancing category there were no respondents then the sum of the weights of the nonrespondents would be proportionately allocated over all balancing categories at that level in the hierarchy (within project, stratum, and grade). If there was at least one respondent in a category then no collapsing of categories to the next higher level was necessary. In no case during the weight adjustments was it necessary to apportion weights to a different project.

VI. SUMMARY OF WEIGHT ADJUSTMENTS

The actual techniques employed in weight adjustments have been described in previous sections of this appendix; however, the order in which the various techniques were applied has not been addressed. Weight adjustment was a sequential process, and the nature of that

Figure E.1

HIERARCHICAL DEFINITION OF WEIGHTING CATEGORIES FOR STUDENTS
(Only One Complete Path is Illustrated)


UB/CS
UB (u)
CS (c)
PROJECT STRATUM (h)
1
2
3
. . . . .
27
PROJECT WITHIN STRATUM (i)
1
2
GRADE (g)
10
11
12
BALANCING CATEGORY (k)
1 2 3 16
STUDENT (j)
1 2 3 . . . . $n_{ugk}(hi)$

sequence is important to an understanding of the basic character of the resultant weights used in the various analyses summarized in Chapters 4 and 5 of this report.

There were six basic steps in producing the 22 intermediate and analysis weights of this study. At each step, the weight resulting from the previous step was adjusted by a particular technique; thus, after step 1, the input to any of the adjustment techniques employed was a previously adjusted weight. Due to the basic differences in the three adjustment techniques defined above, applying these three techniques in different orders will typically result in different weights. Moreover, some steps in the adjustment process involved removal of certain individuals from the data base (i.e., setting their weights to zero), and the stage at which such individuals were removed would affect the final weight distribution among those remaining.

The first step was adjustment of the base-year raw sampling weights for the sample ineligibility of 67 individuals. These individuals consisted of:

a) 21 nonparticipants from a single project for whom names had never been established during the base-year study due to high school refusal;

b) 2 UB participants from a single project, whose names had never been established in the base-year study, due to an operational oversight during field operations;

c) 40 individuals who had been identified as either not in high school or in a grade level less than 10 at time of sampling for the base-year study;

d) 1 individual who had been represented by duplicate records in the base-year file (both as a UB participant and as a nonparticipant--the nonparticipant record was removed); and

e) 3 individuals determined to be deceased through mail returns.

Those cases in categories (c) and (d) above were clearly not defined within the sampling frame of the base-year study, and the associated weights were set to 0. Those in categories (a) and (b) should have been available for followup but clearly could not be reached, and the sampling weights associated with these cases were therefore appropriately redistributed to other students in the sample.[18] The deceased sample members in group (e) were considered to be no longer in the population of interest, and their weights were set to zero.

The second weight adjustment step consisted of adjustment for the subsampling of mail nonrespondents for telephone interview, using the procedure specified in Subsection V.A of this appendix. This adjustment step was confined to the subset of individuals who had not responded by mail at the time of subsampling and resulted in setting to zero the weights of 1,550 nonselected individuals (875 from the UB group and 675 from the comparison group) while increasing the weights of those remaining by the appropriate stratum factor.

The third step in the process involved adjustment for "instrument nonresponse," which in this case was equivalent to a telephone refusal or a failure to contact by telephone. This adjustment used the procedures defined in Section V of this appendix and involved an apportioning of weights of 202 instrument nonrespondents (77 from the UB group and 125 from the comparison group) to individuals most like them.

The fourth step involved producing the two balanced weights for the comparison group (Section III, above). This step, unlike previous steps, did not result in setting any additional weights to zero. It is important to note that at this step the two groups (former participants and nonparticipants) were equated on the marginal distributions of the factors used in the balancing, but that in subsequent adjustment steps such equality was not strictly maintained (due to removal of certain records and collapsing of categories in nonresponse adjustments). The decision to perform the balancing at this stage rather than at a subsequent stage was not an arbitrary one. One of the requirements of the balancing procedure is sufficient category sizes to produce stable proportions, and additional adjustments prior to balancing would have resulted in a reduction of some category sizes to marginal lower limits (necessitating an additional collapsing of balancing categories).

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[18] Weights for those in group (a) were apportioned to comparison students in the remaining feeder high school associated with the project involved (this operation had actually taken place during the base-year study), while weights for those in group (b) were redistributed to similar former participants from the same UB project (as specified in Subsection V.B above).

The fifth step in the adjustment process involved the removal of additional "ineligible" individuals from the data base (i.e., setting their weights to zero). The specific individuals removed were: (a) those who were reported as deceased by telephone (3 UB participants and 7 from the comparison group) and (b) those with UB participation classification conflicts. Since those selected for telephone interview were to represent those not so selected and since those for whom information was collected by telephone represented those for whom telephone data were not collected, the deceased sample members were removed at this step (after adjustments for subsampling and noncontact) rather than at the earlier step in which other deceased sample members (reported by mail return) were removed.

The removal of cases with conflicting UB participation classification had not been anticipated. Once the data were on file, it was determined that some individuals who were originally selected into the UB group now indicated they never participated in UB and that some individuals who were originally selected into the comparison group now stated UB membership (in some cases prior to the original sampling). The first group (previously classified as UB) consisted of 37 individuals who were either inappropriately defined in the sampling frame or who did not report their UB participation for some reason (forgot about it or did not wish to report it). Various forms of bias could have entered the results by inclusion of these cases (the type of bias suspected varies with the reason for the conflicting classification), and exclusion of the cases seemed justified.

The second group (previously classified in the comparison group) consisted of 193 individuals and could be subdivided into two categories. The first category included those who reported first UB participation in a grade prior to their grade at time of sampling. If their follow-up questionnaire responses were correct, then they were probably not defined in the original sampling frame (e.g., were not active UB participants at time of sampling and were not in the comparison population due to prior UB participation). Other explanations are possible, but exclusion of this group seemed warranted. The second category contained those who reported first UB participation in their grade at time of sampling or subsequently. If the follow-up questionnaire responses were correct, then this group likely represents those who were nonparticipants at time of sampling but who joined UB after the time at which the sample was defined. Original plans had called for inclusion of this group in analysis; however, on examination of sample weights, it was discovered that this small group carried considerably more weight than the entire original UB sample. This group was not large enough to be included as a separate analysis category, and, so as not to distort the results, it was decided to exclude this group also.

The final step in the weight adjustment process involved adjustments for item nonresponse or indeterminancy (see Appendix D). Most of the analyses reported in Chapter 4 involved classification of individuals as to extent of prior UB participation and, in some cases, as to initial grade cohort; adjustments for absence of this classifying information was therefore required. Additionally, for each outcome measure examined in Chapter 4 adjustments were required to correct for lack of information on those measures. In all, ten analysis weights were developed for the analyses reported in Chapter 4. The analyses reported in Chapter 5 also necessitated weight adjustments, in that the regression techniques used required complete data for the entire predictor variable set as well as for the criterion variable. A total of three analysis weights were produced for the analyses reported in Chapter 5, one for each criterion variable.

Table E.3 presents a summary of the 22 weights produced, including summary statistics of the weights within the UB and comparison groups. This presentation clearly shows the shrinkage of the effective sample size during the several adjustment steps. The table also demonstrates the typically greater disparity between individual weights as more and more adjustments were made; statistical compensation for such disparities typically resulted in larger standard errors.

Table E.3

SUMMARY OF WEIGHT ADJUSTMENTS

				SUMMARY STATISTICS[b]							
				Former Participants				Nonparticipants			
Weight	Step in Which Produced[a]	Input Weight for Adjustment	Adjustment	Number of Nonzero Weights	Weight Sum	Minimum Weight	Maximum Weight	Number of Nonzero Weights	Weight Sum	Minimum Weight	Maximum Weight
W0	0	--	None (Raw Sampling Weight)	3710	21,145	2.6	10.3	2319[c]	1,410,878	18.0	7,394.3
W1	1	W0	Sample Eligibility	3667	20,895	2.6	10.3	2316	1,407,969	18.0	7,394.3
W2[d]	2	W1	Telephone Subsampling	2792	20,886	2.6	68.2	1641	1,413,094	18.3	15,694.0
W3	3	W2	Telephone Noncontact	2715	20,886	2.6	68.2	1516	1,413,094	18.3	12,567.5
W4	4	W3	Balancing Within Grade	2715	20,886	2.6	68.2	1516	1,413,094	2.1	22,447.3
W5	4	W3	Balancing Over Grade	2715	20,886	2.6	68.2	1516	1,413,094	.8	34,861.0
W6[e]	5	W3	Remove Other Inelligibles	2675	20,592	2.6	68.2	1316	1,231,788	19.3	11,819.3
W7[e]	5	W4	Remove Other Inelligibles	2675	20,592	2.6	68.2	1316	1,185,034	2.1	22,447.3
W8[e]	5	W5	Remove Other Inelligibles	2675	20,592	2.6	68.2	1316	1,207,569	.8	34,861.0
W9	6[f]	W7	Item Indeterminacy (UB Participation)	2656	20,592	2.6	75.8	1277	1,185,034	2.1	24,041.9
W10	6[f]	W8	Item Indeterminacy (UB Participation)	2656	20,592	2.6	75.8	1277	1,207,569	.8	37,337.6
W11	6[f]	W9	Item Indeterminacy (Educ. Progress)	2610	20,592	2.6	80.6	1267	1,185,034	2.1	24,041.9
W12	6[f]	W10	Item Indeterminacy (Educ. Progress)	2610	20,592	2.6	80.6	1267	1,207,569	.8	37,337.6
W13[g]	6[f]	W9	Item Indeterminacy (Highest Grade given PSE entry)	1864	14,276	2.6	80.6	634	574,514	2.1	24,041.9
W14[g]	6[f]	W10	Item Indeterminacy (Highest Grade given PSE entry)	1864	14,276	2.6	80.6	634	630,754	.8	37,337.6
W15[g]	6[f]	W9	Item Indeterminacy (Current Enrollment given PSE entry)	1852	14,259	2.6	80.6	636	575,836	2.1	24,041.9
W16[g]	6[f]	W10	Item Indeterminacy (Current Enrollment given PSE entry	1852	14,259	2.6	80.6	636	632,307	.8	37,337.6
W17[g]	6[f]	W9	Item Indeterminacy (Lag entry)	468	4,481	2.6	54.8	232	211,008	8.5	3,542.4
W18[g]	6[f]	W9	Item Indeterminacy (Less-than-normal Educational Progress)	1279	10,933	2.6	85.3	785	817,490	2.1	25,522.3
W19	6[h]	W6	Item Indeterminacy (Educational Progress)	2072	20,592	2.6	85.3	1120	1,231,788	19.3	12,420.7
W20	6[h]	W6	Item Indeterminacy (Postsecondary Entry)	2080	20,592	2.6	85.3	1123	1,231,788	19.3	12,420.7
W21	6[h]	W6	Item Indeterminacy (Educational Aspirations)	1920	20,592	2.8	85.3	1074	1,231,788	19.3	15,982.9

[a] Steps referenced in this column are those described in the text.

[b] Weights were carried to two decimal places, but have been rounded for this presentation.

[c] Weights of 21 individuals in the comparison group, from a high school that refused to participate, were set to zero and redistributed during the the base-year study.

[d] The procedure used results in equal weight sums only in a stochastic sense.

[e] Although the sums of the comparison group weights (W3, W4, and W5) are identical, the respective adjusted weights (W6, W7, and W8) do not have equal sums, even though the same individuals were removed. This is due to the fact that the weights for a given comparison individual for W3, W4, and W5 are different due to balancing; thus, removal of a given subset of individuals will typically remove different weight totals from the 3 weight sums (W6, W7, and W8). Note that UB weight totals for W6, W7, and W8 are constant as they should be.

[f] For Chapter 4 analysis.

[g] All of the variables referenced for these adjustments were defined for only a subset of individuals, thus reductions in weight totals are expected.

[h] For Chapter 5 analysis.

Appendix F

Procedures and Results of Comparative Descriptive Analyses

Appendix F

Procedures and Results of Comparative Descriptive Analyses

I. GENERAL

This appendix particularizes the comparative descriptive analyses leading to the findings reported in Chapter 4 of this report. Specifically emphasized are: (a) the general approach to analysis, (b) constraints and limitations of the analyses, (c) the relationship of the analyses performed to the specific analysis questions addressed by the study, and (d) tabular presentations of analysis results. Topics that are closely related to these analyses are detailed elsewhere: standard error computation, in Appendixes A and E; weighting adjustments, in Appendix E; and data reliability and analysis variable specifications, in Appendix D.

The comparative descriptive analyses were similar in form to the analyses of participant and nonparticipant differences performed in the base-year study.[1] Using the student as the unit of analysis, aggregate differences in the outcome variable measures were examined as a function of extent of UB participation, with statistical control of a set of concomitant variables (specifically, student background factors and initial grade distributions). The results of these analyses reflect the overall relationship between participation in the UB program and student outcomes, controlling for differences in input to the program. The technique of statistical adjustment for preexisting differences between UB participants and nonparticipants was one of balancing. Other sampling weight adjustments were made for telephone interview subsampling and for nonresponse. These techniques are analogous to those employed in the base-year study. Each of the analysis questions of this study was addressed by these analyses, and the nature in which each question was approached is detailed in Section III below.

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[1] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-152)

In general, the comparative descriptive analyses were restricted to the examination of single-degree-of-freedom contrasts of relevant means or proportions for a given outcome measure. Omnibus tests for main effects or interactions were not implemented; rather, meaningful differences in percentages or means between various subpopulations were considered. For each difference (single-degree-of-freedom contrast) an appropriate standard error was calculated, allowing for sampling effects (see Subsection IV.B of Appendix A and Section IV of Appendix E), to provide a confidence limit for the difference.

To provide unbiased estimates of population parameters, sampling weights (the inverse of the probability of selecting the individual) were used in analyses. The use of sampling weights overcomes possible bias in resulting statistical estimates which may be introduced because individuals were selected with unequal probability (due to oversampling of specific groups). Two conceptually different weight adjustment procedures were used to achieve more meaningful results. The first procedure (sometimes called standardization or balancing) is a technique whereby an artificial nonparticipant population was constructed to be directly comparable to the UB participant group on relevant student background variables (sex, race, poverty status, and academic risk status) and on initial grade cohort distribution.[2] In essence, this procedure forces an equivalence of the two groups in terms of characteristics that were presumed to be related to outcomes but not completely controlled by the study design (see Appendix E, Sections II and III). The second procedure involved the adjustment of sample weights for the sampling procedures used, for instrument nonresponse, and for item indeterminacy. The weight adjustment approach, which involved apportioning of sampling weights of nonselected and nonresponding individuals to those of selected and responding individuals, was chosen over several less conservative imputation approaches (see Appendix E, Section V).

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[2] The comparison population was defined as all non-UB students enrolled in the tenth, eleventh, or twelfth grade of high schools served by a UB project in September 1973. There was no reason to believe, however, that the distributions of this population by grade or by relevant background characteristics would be exactly the same as those of the UB student population, and, as shown in Table F.40 of this appendix, the groups differed markedly on some such factors.

II. CONSTRAINTS AND LIMITATIONS OF ANALYSES

Care was taken throughout analysis to avoid comparisons that would introduce bias in results that were either favorable or unfavorable to the UB program, and considerable effort was made to ensure that the data entering analysis were error free and consistent (see Appendix D). The original sample was designed to reduce many of the possible differences which could exist between the UB and comparison samples (and which could, therefore, lead to spurious group differences). Additionally, statistical equalization of the two groups was undertaken in an attempt to reduce possible contamination of the results by existing differences between groups on background factors and initial grade distribution. Weight adjustments for indeterminate response at the instrument and item level were employed in an effort to avoid distortion of the findings due to incomplete data collection. Standard errors were computed for all major differences so that probability statements concerning the populations of interest could be made and "significant" differences could be uncovered.

Although it is quite likely that these efforts were at least partially successful, they did not ensure removal of all possible confounding factors, nor did they eliminate all constraints and limitations on the study findings. Two particularly difficult remaining confounds are associated with the relationships between extent of UB participation and outcome measures of educational progress or persistence. There are also specific constraints introduced for reported analyses that are restricted to response-defined subsets of sample members (e.g., those enrolling in PSE). Further, two basic problems inherent in questionnaire surveys remain: namely, questionable validity of reported information and the inability to definitively establish causation. The major limitations on the study findings are considered below in some detail.

A. Validity of Data

The preponderance of data for this study is based on unverified student self-reports or, in some cases, secondary source reports. In addition to known biases in student reports (of such matters as high school grades, parent income, parent education level), invalid responses may be traced to a diversity of possible sources (e.g., deliberate attempts to mislead, lack of understanding or misinterpretation of questions, and simple errors in marking responses). Additional (or different levels of) errors are introduced when secondary source data are considered. No direct validation of collected data was attempted; validation studies, while theoretically very important, are typically not cost-feasible. Two indirect validation efforts were attempted, involving examinations of data reliability (a necessary but insufficient condition for validity). One such examination involved the comparison of responses on duplicate returns; the other involved checks for consistency of follow-up data with base-year data. Both of these reliability studies are detailed in Appendix D.

Examinations of duplicate returns indicated a relatively high degree of agreement in reports of relatively objective information (e.g., highest grade, extent of UB participation, current enrollment) but considerably less agreement in reports of more subjective information (aspirations, problems experienced), reflecting in part the temporal instability of these latter measures. Moreover, the agreement between responses on two mail returns was generally greater than that between mail and telephone responses, suggesting some misinterpretation of questionnaire items when the added explanatory power of interview was not available. Finally, there was generally no more than moderate agreement between student self-reports and secondary source reports within the small subset of cases for which such comparisons could be made. These findings signal caution in interpreting some of the study results.

Examinations of consistency of follow-up data with base-year data were possible for a few items of information (extent of UB participation and highest grade completed). There were only 10 cases (less than one-fourth of one percent) in which report of highest grade completed was inconsistent with previously collected data; however, the consistency checks involved here were only capable of detecting very large response discrepancies (see Appendix D). On the other hand, there were 1,245 instances (almost 30 percent) of inconsistency of reported extent of UB participation with previously established participation. Although this figure may be somewhat deceptive,[3] it nevertheless signals the possibility of a considerable lack of reliability in the retrospective measure of extent of program participation.

The problem of data validity may have been compounded through the weight adjustment procedures, particularly if invalid data were collected from a student who was similar to a large group of nonrespondents. The real danger of the validity problem, however, is that of differential validity of responses within the two groups (UB and comparison). While a lack of reliability and validity serves to introduce error into the data, if this error is the same (regardless of direction or magnitude) within the UB and comparison groups, then group *differences* will show no directional bias. If, on the other hand, the data from one group are biased in a particular direction and those from the other group are unbiased (or biased in the opposite direction), then the bias will remain (or be magnified) in the group differences. None of the reliability or consistency checks were sufficient to address this issue, and, while there is no reason to suspect differential response biases, the possibility of this source of error in group differences does exist.

B. Establishing Causation

A logical error which is made with sufficient frequency to warrant discussion is that of inferring causality from an established relationship. While the existence of a relationship between two variables is a necessary condition for the existence of causation, it is not a sufficient one. In addition to a relationship between two variables (conditions), establishment of causation requires that: (1) the causal variable (antecedent condition) logically precedes the effect (consequent condition); and (2) no other antecedent variable exists which may explain both the antecedent and consequent variables under consideration. While the first of these requirements is often fairly easy to establish, the second is extremely difficult to establish outside the true experimental paradigm. Although advances have been made recently in the area of causal modeling for nonexperimental or quasiexperimental research paradigms, the assumption of inclusion of all relevant variables in the model is a basic tenet in these approaches.

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[3] Only 100 of these cases represented a conflict in whether the individual had or had not participated in UB; the majority of conflicts involved the length of participation among those who had participated. It was suspected that these latter inconsistencies were a reflection of the imprecision of the previously derived base-year measure (an index specifying *grades* in which participation had occurred that had been derived from reported *date* of first participation).

In regard to educational intervention programs, it is extremely difficult, if at all possible, to identify (much less to measure) all the relevant student, process, and contextual variables which may influence one another and/or student outcomes. The UB program is no exception to this constraint. Moreover, the nature of sample selection and measurement used in the UB evaluation study was such that it was not possible in all cases to determine even the temporal order of occurrence of two events. Even when strong associations exist between previous UB participation and current outcomes, the possibility still remains that these associations are largely a function of other unconsidered (or unmeasured) antecedent variables which are related both to current outcomes and to selection of UB participants.

C. Confounding of Extent of UB Participation and Educational Progress and Persistence

As specified more fully in Section III of this Appendix, most analyses involving educational progress/persistence were conducted within initial grade cohort and involved a partitioning of the former UB participant group on the basis of the number of years during which they had participated in UB. While comparisons of the total UB participant group with the nonparticipant group are relatively straightforward, comparisons within the UB group (among subgroups with differential exposure to the program) are not as meaningful for two basic reasons. First, for reasons noted in Subsection II.D below, the three exposure groups (1, 2, or 3 years' exposure to the program) were not equated on background factors expected to influence educational progress/persistence (e.g., sex, race, poverty status, academic risk status, and initial grade cohort), since balancing was not performed within the UB group. While differences among the three exposure groups were minor in most cases (less than five percentage points) as shown in Table F.41 below, most of the differences were statistically significant ($p<.05$). Groups with greater exposure to the program contained larger proportions of younger individuals and blacks, as well as smaller proportons of academic risks.

Second, there are subtle confoundings of extent of UB participation and educational progress. One type of confounding that exists is related to an indeterminacy of antecedent and consequent condition (see Subsection II.B above). While longer participation in the program may be seen as theoretically preceding further educational progress, the opposite can occur, in that dropping out of high school (cessation of high school progress) leads to termination from the program (limitation of program participation). This directional constraint on the relationship is most pronounced for students in the tenth- and eleventh-grade cohorts.

Another type of confounding is related to the so-called "survivor effect," which is a form of group self-selection. As an example, consider the comparison of the educational progress of two groups of twelfth graders, one group consisting of individuals who joined UB for the first time during their senior year and the other consisting of those who participated in UB in the tenth, eleventh, and twelfth grades. The groups being compared would be basically different (even if equated on certain background factors), since one is comparing students who "survived" on their own in a school system through which the other students had been specifically assisted (i.e., to become seniors). If this survivor effect is large, then valid comparisons could only be made among those joining the program in the same grade or after controlling for the effect. Such control has not been accomplished in this study except within the tenth-grade cohort.[4] There is an obvious interaction between this confound and that discussed in the previous paragraph.

D. Loss of Statistical Control in Analyses Involving Response-Determined Subgroups

Statistical control for certain background factors (race, sex, poverty status, academic risk status, and initial grade cohort) was initiated through a weight adjustment process that equated the UB and comparison groups on the distribution of those variables (see Appendix E, Sections II and III). One of the prerequisites of this process is a sufficient numbers of individuals within the defined balancing categories to obtain stable estimates for balancing. This requirement precluded balancing within the UB participant group on the basis of extent of participation or within other response-defined subgroups (e.g., the subset of individuals who were currently enrolled in school).

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[4] Strictly speaking, this confound is also present for comparisons between UB participants and nonparticipants in grade cohorts 11 and 12. In this respect, the Markov model approach used in the base-year study was theoretically more appropriate, despite its acknowledged weaknesses.

Since many of the analyses conducted were relevant to (or defined in terms of) only a specific subgroup of students, it is important to note that some degree of statistical control was lost in these analyses. The two most frequent subgroups involved were: (1) subgroups with different length of participation within the UB group (as discussed in the previous subsection) and (2) the subgroup of individuals (both UB and comparison) who entered postsecondary education (PSE). The latter subgroup is one of particular interest, since PSE persistence indices were defined for this subgroup exclusively. While former program participants and nonparticipants were statistically equated in a global sense, the statistical equivalence does not necessarily hold after the imposition of another condition (specifically entry into PSE) for further partitioning. In fact, the subgroup of comparison students entering PSE differed significantly from the subgroup of former UB participants entering PSE in most respects (see Table F.8 in Section IV below). Specifically, the PSE-entering subgroup of nonparticipants contained smaller proportions of: (1) older individuals, (2) ethnic minorities, (3) males, (4) poverty level individuals, and (5) academic risks. Thus, comparisons made between participants and nonparticipants who entered PSE should take into account these differences, which theoretically favor the comparison group in terms of progress and persistence measures.

III. ANALYTIC APPROACH

The basic analytic approach used for the comparative descriptive analyses was specified jointly by RTI and OPBE during an early stage of the study. These specifications were intended to facilitate actual conduct of analysis but were not intended to constrain investigations;

thus, some deviations from the original plan were realized once data were in hand and analysis had begun. Analysis was constrained, however, by the general design of the base-year study (see previous section), by the limited set of data collected (see Appendixes B and D), and by the specific analysis questions to be addressed by the study.

A. Analysis Questions

The analysis questions addressed in the comparative descriptive analyses are presented in Chapter 1 of this report, but they are repeated below for ease of reference.

1) Do rates of PSE persistence and of educational progress differ as a function of extent of UB participation?
    a) Are persistence differences between UB and comparison students a function of the level of offering of PSE institutions attended (e.g., two-year or four-year)?
    b) Are persistence differences between UB and comparison students a function of either UB host status or Special Services for Disadvantaged Students (SSDS) host status of institutions attended?
2) For those who were high school seniors in 1973, do the PSE persistence and "lag entry" rates depend upon length of participation in UB? For those who were high school sophomores and juniors in 1973, does less-than-expected educational progress depend upon the length of participation in UB?
3) Do PSE persistence rate differences depend upon adequacy of financial aid?
4) For those who were high school sophomores and juniors in 1973, do the PSE entry rates differ for UB and comparison students?
5) Do the institutional characteristics (e.g., highest offering, cost, size, residence characteristics, composition of student body, availability of SSDS, UB host status) of PSE institutions entered by UB students differ from the characteristics of PSE institutions entered by comparison students?
6) Does the current work status of UB participants differ from that of comparison students?

7) Does the current military service status of UB participants differ from that of comparison students?

8) Do the educational expectations of UB participants differ from those of comparison students?

9) Do the problems experienced by UB participants in obtaining their desired education differ from those experienced by comparison students?

B. Analysis Variables

The specific analysis variables generated to address these questions are detailed in Appendix D; however, they are listed below for ease of reference:

1) Characteristics of PSE institutions attended (a set of indices constructed from an institutional file and matched to the specific student through his specification of institution last attended).

2) Highest grade completed (a measure of educational progress/persistence obtained directly from responses to items 1 and 5 of the Follow-Up Questionnaire).

3) High school completion (an index of educational progress/persistence obtained as a dichotomization of highest grade completed).

4) PSE entry (an index of educational progress/persistence obtained as a dichotomization of highest grade completed).

5) Highest grade completed given PSE entry (a measure of educational progress/persistence identical to (2) above but conditional on PSE entry and, thus, defined only for that subgroup of individuals for whom highest grade completed was greater than 12).

6) Enrollment status given PSE entry (an index of educational progress/persistence obtained from item 4 of the Follow-Up Questionnaire and defined only for that subgroup of individuals who had completed education beyond the twelfth grade).

7) Lag entry into PSE (an index of educational progress identical to PSE entry, (4) above, but defined only for that subgroup of

individuals who had completed grade 12 as of fall 1974 and who had not entered PSE at that time).

8) Less-than-normal educational progress (an index of educational progress/persistence that was defined in terms of highest grade completed, (2) above, and expected normal uninterrupted progression through high school and postsecondary grade levels--defined only for those who were in grade cohorts 10 and 11).

9) Current work status (an index derived directly from items 2 and 3 of the Follow-Up Questionnaire).

10) Current military status (an index derived directly from item 3 of the Follow-Up Questionnaire).

11) Educational aspirations (a measure derived from item 6 of the Follow-Up Questionnaire).

12) Deterrents to education (a set of indices obtained from item 7 of the Follow-Up Questionnaire).

13) Extent of UB participation (a classifier obtained directly from the responses to item 8 of the Follow-Up Questionnaire).[5]

14) Initial grade cohort (a classifier obtained from the base-year master file).

In regard to the various indices and measures of educational progress/persistence, it should be noted that the clear division of these two concepts is extremely difficult, and particularly so for the data set to which variable construction was limited. A measure of educational progress can be obtained which is effectively independent of the educational goals of the individual; however, this measure of educational progress will not be independent of persistence, since persistence is a necessary (but not sufficient) condition for progress. A true measure of educational persistence would ideally depend upon an individual's educational goals, but these are not sufficiently obtainable from the current data base. Moreover, since progress implies persistence, a measure of progress is also a measure of persistence. In all cases, the analysis indices labeled persistence reflect some sort of progress (even the conditional measures). Thus, it is theoretically unsound, using the analysis variables constructed, to speak of progress and persistence as independent concepts. In effect, each measure reflects both of the underlying theoretical concepts.

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[5] Original plans had called for augmenting this variable, where needed, by a similiar index derived in the base-year study; however, due to the considerable lack of agreement between the two measures (see Subsection II.A of this Appendix), that plan was not implemented. Even though data for cases of inconsistency were flagged as inconsistent in the master file, they were used in analysis.

C. Analyses Performed in Relation to Analysis Questions

The first analysis question concerns persistence in PSE and rates of educational progress. As indicated in the previous subsection, there are several measures of progress/persistence which in actuality cannot be completely separated. In addressing the main analysis question, five measures of progress/persistence were used: highest grade completed, high school completion, PSE entry, highest grade given PSE entry, and current enrollment given PSE entry. The last three measures deal exclusively with persistence after high school and express somewhat different aspects of persistence. These variables were examined as a function of both 1973 grade level and extent of participation in UB as well as in the aggregate (over grades and over extent of UB participation within the participant group). The standard format within which such analysis questions were addressed is reflected in Table F.1.1 of Section IV below. The major comparisons of interest both within grade cohort and in the aggregate over grades[6] were: (1) total former UB participant group contrasted with nonparticipant group, (2) one-year former UB participants contrasted with more-than-one-year former participants, and (3) two-year participants contrasted with three-year participants. Tabular summaries of the results of these analyses, together with supporting results for aid in interpretation, are presented in Tables F.1.1 through F.5 and Tables F.8 through F.10 of Section IV of this appendix.

Since the subquestions, 1.a and 1.b, were meaningful only for the subgroup of individuals entering PSE, they were examined using the two conditional analysis variables, highest grade given PSE entry and current enrollment given PSE entry. It was originally planned to conduct all such analyses within grade-cohort, since the value of highest grade was restricted in the range that it could assume, depending upon initial grade.[7] The original plan proved untenable in the case of institutional level of offering, due to the fact that a large number of attended institutions could not be classified on that index. As a result, a four-valued classification scheme was devised, considering: (1) indeterminate classification institutions, (2) two-year institutions, (3) four-year colleges, and (4) universities. This classification, unfortunately, did not allow sufficient cell sizes to produce stable estimates when considered within grade cohort, and, although the data are presented within grade cohort (Tables F.11.2 and F.12.2), standard errors were not produced for these estimates. Alternately, differential persistence as a function of institutional level of offering was analyzed for the aggregate groups (after balancing over grade cohorts) by extent of former UB participation. The contrasts of interest were identical to those specified in the previous paragraph, and the tabular presentations are given in Tables F.11.1 and F.12.1 of the following section.

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[6] The comparison set specified is but one of many sets of independent comparisons possible. The particular comparisons were chosen as a priori contrasts primarily on the basis of their meaningfulness in relation to UB program operation. The weights used in within-grade comparisons differed from those used for aggregate comparisons, reflecting the separate balancing procedures (see Appendix E).

It was possible to perform analyses related to question 1.b within grade, as planned. Within a grade (as well as in the aggregate) and institution type (host or nonhost), the comparison of interest was the total former participant group contrasted with the nonparticipant group. Summaries of these analyses are presented in Tables F.13 through F.16.

The second analysis question deals with the persistence and lag entry rates of 1973-74 high school seniors and with less-than-expected educational progression of 1973-74 high school sophomores and juniors.

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[7] It should also be noted that both outcome variables considered in these analyses are theoretically confounded with the classifiers of type of PSE institution attended. Highest grade completed is restricted to a lower value given entry into a two-year institution as opposed to a four-year institution. Further, normal progression by a student, who was in grade 12 in Fall 1973, in a two-year college would have led to the completion of his course of study and consequently to no current enrollment. Moreover, since UB and SSDS host institutions are placed in four-year institutions in disproportionately large numbers relative to the population proportion of such institutions, the confoundings are extended to those PSE classifiers also.

The comparison of educational progress/persistence rates of 1973-74 seniors in the UB and comparison group was treated in addressing analysis question 1. The remaining question concerns the rate of lag entry of those seniors who did not immediately enter PSE on completion of high school. The analysis was performed within the twelfth-grade cohort, and the comparisons of interest were the same, within that grade, as those for analysis question 1. Table F.6 presents the results of this analysis. Similarly, educational progress/persistence of 1973 high school sophomores and juniors has already been examined, and the question remaining concerns differences in less-than-expected educational progression. The analyses were conducted within the tenth- and eleventh-grade cohorts, and the contrasts of interest were the same as previously described for lag entry. Results are presented in Table F.7.

Analysis question 3 concerns the possible moderation of PSE persistence rate differences as a function of adequacy of financial aid. Since the concern is with PSE persistence, the conditional measures, highest grade given PSE entry and current enrollment given PSE entry were used in addressing this question. The educational deterrent index for financial aid was used as a classifier in these analyses after aggregating over grade cohorts. In both analyses the comparisons of interest, within each level of the financial aid deterrent index, were the same as those listed for analysis question 1. Tabular summaries are given in Tables F.17 and F.18.

Analysis question 4 concerns the PSE entry rates for UB and comparison students in the tenth- and eleventh-grade cohorts. This question was answered in addressing analysis question 1, using PSE entry as a measure of progress/persistence (see Tables F.3 and F.4).

Analysis question 5 concerns possible differences in the distributions of UB and comparison students among various types of PSE institutions as defined by the institutional indices constructed for this study. These analyses were necessarily restricted to those individuals who had entered PSE, and involved investigation of differential matriculation patterns for each index separately. Due to restrictions in cell sizes and lack of theoretical justification for within-grade comparisons, all of these analyses were performed for data aggregated over the grade cohorts. For all indices considered, former participants were categorized by the extent of their participation; however, for the majority of indices the only comparisons for which standard errors were computed were those between the total UB group and the comparison group. For four of the indices considered to be of particular policy relevance (involving financial aid characteristics and institutional host status for UB and SSDS programs), standard errors were reported for three comparisons of interest, within the levels of a given PSE index. These contrasts were the same as those specified above for analysis question 1. Summaries of these analyses are presented in Tables F.20 through F.30; Table F.19 presents measures of association between the various PSE indices, to show interdependencies between the various analyses involving specific indices.

Analysis question 6 concerns possible differences between UB participants and nonparticipants in terms of their current work status. The question was addressed for each of two mutually exclusive subgroups, those currently enrolled and those not currently enrolled; since it was felt that interpretation of group differences regarding work status would be considerably different, depending on the subgroup involved. Although initial grade cohort may have been related to work status (for both the in-school and out-of-school subgroups), within cohort comparisons were seen as providing only marginally interesting results; therefore, these analyses were performed after aggregating across grade cohort. The UB group was partitioned by extent of participation, and the comparisons of interest, within a given work status category, were the same as those specified above for analysis question 1. Summaries of these analyses are given in Tables F.31.1 and F.31.2.

Analysis question 7 concerns possible differences between UB participants and nonparticipants with respect to military service status. Since all initial grade cohorts would have finished high school with normal progression, the analysis was performed after aggregating across grade cohorts. For this analysis, UB participants were not further partitioned by extent of their participation; therefore, the comparison of interest, within each category of military service status, was that between the total UB group and the nonparticipant group. The results of this analysis are summarized in Table F.32.

Analysis question 8 concerns possible differences in the educational expectations of former UB participants and those of nonparticipants. Initial grade cohort was not seen to be a particularly relevant factor with respect to educational aspiration (all cohorts should have finished high school at the time of the survey), and this analysis was performed after aggregating over initial grade. In this analysis, UB participants were partitioned by the number of years of participation. The comparisons of interest, within the various levels of aspiration, were the same as those specified for analysis question 1, and results are summarized in Table F.33.

Analysis question 9 concerns possible differences in the problems experienced by former UB participants and nonparticipants in obtaining their desired level of education. Separate analyses were performed for each of the six educational deterrents listed on the Follow-Up Questionnaire. Even though there are theoretical reasons for expecting different responses as a function of initial grade cohort, for all but one of these deterrents analysis was performed after aggregating over initial grade. Also, in all but one of the analyses the UB group was not further partitioned by number of years of participation. For these analyses, the comparison performed contrasted the total UB participant group with nonparticipants. One potential educational deterrent, adequacy of financial aid, was considered to have important policy implications. For that case, analysis was performed within each grade cohort as well as in aggregate, and the UB group was partitioned by extent of participation. The comparisons of interest were the same as those specified for analysis question 1. Analysis summaries are presented in Tables F.34 through F.39.

IV. TABULAR RESULTS

The tabular results of the comparative descriptive analyses (and supporting results) are presented in this section. The tables, which constitute the remainder of this appendix, have been annotated sufficiently to be interpreted without reference to textual material.

Table F.1.1

AVERAGE HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

High School Grade Cohort[a]	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
10th Graders	12.39 (N=161)	12.25 (N=105)	11.68 (N=101)	12.16 (N=367)	11.94 (N=419)	0.10* 0.13* 0.16
11th Graders	12.98 (N=338)	12.72 (N=471)	12.39 (N=226)	12.73 (N=1035)	12.35 (N=428)	0.09* 0.09* 0.07*
12th Graders	13.40 (N=428)	13.25 (N=529)	12.94 (N=251)	13.24 (N=1208)	12.93 (N=420)	0.14* 0.12* 0.07*
Total Group[c]	13.07 (N=927)	12.93 (N=1105)	12.51 (N=578)	12.88 (N=2610)	12.57 (N=1267)	0.07* 0.07* 0.05*

NOTE: Table entries are based on weighted data and estimate the cell mean highest grade completed as of February–April 1977. Except as otherwise noted, weights for the comparison groups have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables or highest grade completed. Numbers given in parentheses (N=___) show the total number of cases on which the means were computed.

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

[c] When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.1.2

DISTRIBUTIONAL AND SUMMARY STATISTICS OF HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS: TENTH-GRADE COHORT

Highest Grade Completed	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)
	3 Years	2 Years	1 Year	Total	
10	0.0% (N=0)	5.7% (N=2)	17.3% (N=21)	6.4% (N=23)	13.0% (N=28)
11	6.9% (N=8)	14.8% (N=13)	17.6% (N=13)	12.2% (N=34)	6.8% (N=40)
12	22.8% (N=33)	22.0% (N=31)	36.9% (N=31)	26.4% (N=95)	41.2% (N=167)
12.5	53.1% (N=88)	26.7% (N=24)	19.2% (N=24)	36.1% (N=136)	28.4% (N=126)
13	14.8% (N=27)	23.5% (N=26)	6.8% (N=10)	15.2% (N=63)	8.7% (N=48)
14	2.4% (N=5)	7.3% (N=9)	2.1% (N=2)	3.8% (N=16)	1.3% (N=10)
$N^{a/}$	161	105	101	367	419
$WN^{b/}$	1309	901	821	3031	424,785
Mean	12.39	12.25	11.68	12.16	11.94
Median	12.44	12.39	11.81	12.32	12.05
Standard Deviation	0.55	0.95	0.99	0.86	0.92

NOTE: Distributional percentages are column percentages; these and summary statistics are based on weighted data. Weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. All weights have been adjusted for indeterminancy of classifier variables of highest grade completed. Numbers given in parentheses (N=__) show the number of cases in the associated cell (e.g., the number of cases contributing to the numerator of the percentage).

a/ Total number of cases on which column percentages and summary statistics are based.

b/ Weighted column total (e.g., the sum of weights for the N individuals in a given column).

Table F.1.3

DISTRIBUTIONAL AND SUMMARY STATISTICS OF HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS: ELEVENTH-GRADE COHORT

Highest Grade Completed	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)
	3 Years	2 Years	1 Year	Total	
10	0.0% (N=0)	0.7% (N=3)	4.4% (N=9)	1.3% (N=12)	3.8% (N=13)
11	1.6% (N=5)	6.6% (N=25)	9.7% (N=24)	5.7% (N=54)	10.2% (N=35)
12	17.5% (N=54)	25.9% (N=124)	36.1% (N=81)	25.4% (N=259)	36.3% (N=165)
12.5	11.9% (N=40)	13.1% (N=53)	8.7% (N=22)	11.7% (N=115)	12.3% (N=55)
13	46.3% (N=158)	36.4% (N=178)	29.6% (N=61)	38.1% (N=397)	28.6% (N=120)
14	21.3% (N=77)	15.3% (N=78)	11.2% (N=28)	16.3% (N=183)	8.8% (N=38)
15	1.2% (N=3)	2.0% (N=10)	0.3% (N=1)	1.4% (N=14)	0.1% (N=2)
16	0.3% (N=1)	0.0% (N=0)	0.0% (N=0)	0.1% (N=1)	0.1% (N=0)
N[a/]	338	471	226	1035	428
WN[b/]	2526	3635	1740	7901	392,695
Mean	12.98	12.72	12.39	12.73	12.34
Median	13.06	12.98	12.25	12.87	12.24
Standard Deviation	0.74	0.88	0.96	0.87	0.90

NOTE: Distributional percentages are column percentages; these and summary statistics are based on weighted data. Weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. All weights have been adjusted for indeterminacy of classifier variables or highest grade completed. Numbers given in parentheses (N=____) show the number of cases in the associated cell (e.g., the number of cases contributing to the numerator of the percentage).

a/ Total number of cases on which column percentages and summary statistics are based.

b/ Weighted column total (e.g., the sum of weights for the N individuals in a given column).

Table F.1.4

DISTRIBUTIONAL AND SUMMARY STATISTICS OF HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS: TWELFTH-GRADE COHORT

Highest Grade Completed	Extent of Upward Bound Participation				Comparison Grade (Nonparticipants)
	3 Years	2 Years	1 Year	Total	
11	1.0% (N=4)	3.0% (N=12)	9.8% (N=17)	3.8% (N=33)	4.0% (N=10)
12	15.2% (N=60)	21.8% (N=110)	25.5% (N=66)	20.3% (N=236)	37.8% (N=175)
12.5	11.1% (N=43)	9.3% (N=50)	12.6% (N=34)	10.6% (N=127)	10.2% (N=44)
13	20.4% (N=76)	18.4% (N=90)	15.8% (N=37)	18.5% (N=203)	13.6% (N=63)
14	42.6% (N=196)	37.1% (N=201)	27.7% (N=72)	37.0% (N=469)	26.3% (N=97)
15	9.6% (N=45)	10.4% (N=66)	8.0% (N=23)	9.5% (N=137)	5.7% (N=30)
16	0.2% (N=1)	0.0% (N=0)	0.7% (N=2)	0.2% (N=3)	2.4% (N=1)
N a/	428	529	251	1208	420
WN b/	3387	4207	2066	9660	367,539
Mean	13.39	13.25	12.94	13.24	12.93
Median	13.56	13.39	12.85	13.37	12.65
Standard Deviation	0.94	1.04	1.17	1.05	1.15

NOTE: Distributional percentages are column percentages; these and summary statistics are based on weighted data. Weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. All weights have been adjusted for indeterminacy of classifier variables or highest grade completed. Numbers given in parentheses (N=__) show the number of cases in the associated cell (e.g., the number of cases contributing to the numerator of the percentage).

a/ Total number of cases on which column percentages and summary statistics are based.

b/ Weighted column total (e.g., the sum of weights for the N individuals in a given column).

Table F.1.5

DISTRIBUTIONAL AND SUMMARY STATISTICS OF HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS: ALL GRADE COHORTS

Highest Grade Completed	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)
	3 Years	2 Years	1 Year	Total	
10	0.0% (N=0)	0.9% (N=5)	5.1% (N=33)	1.5% (N=38)	3.8% (N=42)
11	2.3% (N=17)	5.7% (N=50)	10.8% (N=51)	5.7% (N=118)	6.3% (N=84)
12	17.4% (N=147)	23.5% (N=265)	31.5% (N=178)	23.2% (N=590)	37.7% (N=507)
12.5	19.0% (N=171)	12.6% (N=127)	12.3% (N=80)	14.8% (N=378)	13.5% (N=225)
13	28.4% (N=261)	26.4% (N=294)	19.4% (N=108)	25.5% (N=663)	18.8% (N=231)
14	27.9% (N=278)	25.0% (N=288)	16.9% (N=102)	24.2% (N=668)	16.1% (N=144)
15	4.9% (N=51)	5.9% (N=76)	3.7% (N=24)	5.0% (N=151)	2.7% (N=32)
16	0.2% (N=2)	0.[illegible] (N=[illegible])	0.3% (N=2)	0.1% (N=4)	1.2% (N=2)
N[a/]	927	1105	578	2610	1267
WN[b/]	7222	8743	4627	20592	1,207,540
Mean	13.07	12.93	12.51	12.88	12.57
Median	13.05	12.95	12.35	12.89	12.33
Standard Deviation	0.88	1.02	1.15	1.06	1.08

NOTE: Distributional percentages are column percentages; these and summary statistics are based on weighted data. Weights for the comparison group have been balanced to reduce differences from Upward Bound participants on specific background variables and initial grade distributions. All weights have been adjusted for indeterminacy of classifier variables or highest grade completed. Numbers given in parentheses (N=___) show the number of cases in the associated cell (e.g., the number of cases contributing to the numerator of the percentage).

[a/] Total number of cases on which column percentages and summary statistics are based.

[b/] Weighted column total (e.g., the sum of weights for the N individuals in a given column).

Table F.2

HIGH SCHOOL COMPLETION RATES OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

High School Grade Cohort[a]	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
10th Graders	93.1% (N=161)	79.5% (N=105)	65.0% (N=101)	81.4% (N=367)	80.2% (N=419)	c/ 8.18* 7.61
11th Graders	98.5% (N=338)	92.7% (N=471)	85.9% (N=226)	93.1% (N=1035)	86.0% (N=428)	2.84* 3.21* 1.77*
12th Graders	99.0% (N=428)	97.0% (N=529)	90.2% (N=251)	96.2% (N=1208)	96.0% (N=420)	c/ 3.21* c/
Total[d]	97.8% (N=927)	93.4% (N=1105)	84.1% (N=578)	92.8% (N=2610)	89.9% (N=1267)	1.75 2.38* 2.18*

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating completion of twelfth grade or some postsecondary education as of February-April 1977. Except as otherwise noted, weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables or high school completion. Numbers given in parentheses (N=___) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

a/ High school grade in the fall of 1973, at time of initial sample frame definition.

b/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors reported are in the percentage metric. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

c/ Standard errors were not computed for differences less than 2 percentage points.

d/ When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.3

POSTSECONDARY ENTRY RATES OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

High School Grade Cohort[a]	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
10th Graders	70.2% (N=161)	57.5% (N=105)	28.1% (N=101)	55.0% (N=367)	39.0% (N=419)	4.14* 5.08* 8.20
11th Graders	81.0% (N=338)	66.8% (N=471)	49.9% (N=226)	67.6% (N=1035)	49.7% (N=428)	4.65* 4.77* 4.85*
12th Graders	83.8% (N=428)	75.1% (N=529)	64.8% (N=251)	76.0% (N=1208)	58.2% (N=420)	6.07* 5.84* 3.09*
Total [c]	80.4% (N=927)	69.9% (N=1105)	52.7% (N=578)	69.7% (N=2610)	52.2% (N=1267)	4.38* 3.29* 2.54*

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating entry into postsecondary education as of February-April 1977. Except as otherwise noted, weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables or postsecondary entry. Numbers given in parentheses (N=___) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors reported are in the percentage metric. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

[c] When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.4

POSTSECONDARY ENTRY RATES OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO REPORTED HIGH SCHOOL COMPLETION

High School Grade Cohort a/	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference b/
	3 Years	2 Years	1 Year	Total		
10th Graders	75.5% (N=153)	72.3% (N=90)	43.2% (N=67)	67.6% (N=310)	48.6% (N=351)	4.97* 7.32* 7.84
11th Graders	82.3% (N=333)	72.1% (N=443)	58.0% (N=193)	72.7% (N=969)	57.8% (N=380)	4.34* 5.02* 4.75*
12th Graders	84.6% (N=424)	77.5% (N=517)	71.8% (N=234)	78.9% (N=1175)	60.6% (N=410)	5.91* 4.57* 3.20*
Total c/	82.2% (N=910)	74.8% (N=1050)	62.6% (N=494)	75.0% (N=2454)	55.7% (N=1141)	3.06* 3.31* 2.64*

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating entry into postsecondary education as of February-April 1977. Table entries are based on the subset of individuals who reported completing at least the twelfth grade and may include those who did not realize formal high school graduation prior to postsecondary entry. Except as otherwise noted, weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables or postsecondary entry. Numbers given in parentheses (N=___) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

a/ High school grade in the fall of 1973, at time of initial sample frame definition.

b/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors reported are in the percentage metric. Standard errors associated with statistically significant (p <.05) differences are indicated by an asterisk (*).

c/ When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.5

INDICES OF EDUCATIONAL PROGRESS FOR FORMER TWO-YEAR UPWARD BOUND PARTICIPANTS IN TENTH- AND ELEVENTH-GRADE COHORTS, AS A FUNCTION OF THEIR PARTICIPATION DURING THE SENIOR YEAR OF HIGH SCHOOL

Index	High School Grade Cohort[a]	Did Not Participate in 12th Grade	Did Participate in 12th Grade	Standard Errors of Difference
Average Highest Grade Completed	10th Graders	12.04	12.81	0.21*
	11th Graders	12.30	13.00	0.11*
Percentage Completing High School	10th Graders	72.0%	98.8%	9.34*
	11th Graders	83.6%	98.9%	4.23*
Percentage Entering Postsecondary Education	10th Graders	49.2%	79.0%	10.79*
	11th Graders	46.5%	80.5%	5.98*
Number of Cases[b]	10th Graders	69	36	
	11th Graders	180	291	

NOTE: Table entries are based on weighted data and estimate either cell means or percentages as specified in the first column of the table. These values were computed for the subset of former UB participants who were in the tenth and eleventh-grade cohorts and who indicated exactly two years' participation in the Upward Bound program. Weights have been adjusted for indeterminacy of extent of Upward Bound participation or highest grade completed. Standard errors are reported in the metric of the associated statistic. An asterisk (*) is used in the standard error column to indicate a statistically significant (P <.05) difference.

[a] High school grade in fall of 1973, at time of initial sample frame definition.

[b] Numbers given are the total number of cases on which means or percentages were computed (e.g., number of cases contributing to denominators).

Table F.6

PERCENTAGES OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS IN TWELFTH-GRADE COHORT INDICATING DELAYED ENTRY INTO POSTSECONDARY EDUCATION

Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a]
3 Years	2 Years	1 Year	Total		
71.4% (N=152)	61.6% (N=214)	47.8% (N=102)	61.9% (N=468)	36.4% (N=232)	6.85* 9.17 5.91

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating delayed entry into postsecondary education as of February-April 1977. The statistics are based on the subset of former participants and nonparticipants who were high school seniors during the 1973-74 school year and who did not enroll in postsecondary education in fall 1974. Weights for the comparison group have been balanced, within the twelfth-grade cohort, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy or inconsistency of the classifier variables or postsecondary entry. Numbers given in parentheses (N=___) show the total number of cases in which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

[a] Three standard errors are reported, one for each of 3 independent contrasts within the row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

Table F.7

PERCENTAGES OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS INDICATING LESS-THAN-EXPECTED EDUCATIONAL PROGRESS

High School Grade Cohort[a]	Extent of Upward Bound Participants				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
10th Graders	28.8% (N=151)	46.6% (N=95)	72.1% (N=78)	45.0% (N=324)	63.0% (N=386)	4.75* 5.35* 8.96
11th Graders	29.8% (N=330)	44.9% (N=437)	63.4% (N=188)	42.6% (N=955)	60.7% (N=399)	5.02* 4.78* 5.61*

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating less-than-expected educational progress. The values were computed for the subset of former Upward Bound participants and nonparticipants who were tenth and eleventh graders in academic year 1973-74; less-than-expected educational progress was defined in terms of expectations of normal uninterrupted progression through high school and postsecondary grade levels within each grade cohort. Weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables or of highest grade completed. Numbers given in parentheses (N=__) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

Table F.8

DEMOGRAPHIC CHARACTERISTICS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION

Variable	Classification	Former Upward Bound Participants		Comparison Group (Nonparticipants)		Standard Errors of Difference
		Number of Cases[a]	Percentage[b]	Number of Cases[a]	Percentage[b]	
Age[c]	17 or less	19	1.0%	9	0.6%	
	18	121	6.6%	91	6.0%	
	19	436	22.9%	188	29.7%	
	20	686	35.3%	194	33.2%	
	21	484	27.4%	134	27.1%	
	22	89	5.2%	14	2.9%	
	23	12	0.7%	3	0.3%	
	24 or more	3	0.2%	0	0.0%	
	Average	1850	20.02[d]	633	19.90[d]	0.06*
	Indeterminate[e]	14	0.7%	1	0.1%	
Race	Black	1267	67.3%	216	64.0%	1.5*
	White	255	13.0%	337	17.4%	1.0*
	Other	324	18.8%	60	12.2%	1.1*
	Indeterminate[e]	18	0.9%	21	6.3%	1.4*
Sex	Male	776	42.3%	284	38.4%	1.8*
	Female	1088	57.7%	350	61.6%	[f]
Poverty Status[g]	Poverty level	1211	64.3%	247	57.5%	0.8*
	Not poverty level	445	23.9%	373	40.0%	1.8*
	Indeterminate[e]	208	11.8%	14	2.4%	1.6*
Academic Risk Status[g]	Risk	737	42.7%	224	36.0%	1.1*
	Not risk	1118	57.0%	408	63.3%	[f]
	Indeterminate[e]	9	0.4%	2	0.8%	[h]
Total Cases[i]		1864	14,275	634	630,754	

NOTE: Percentages are computed within student groups for the particular variable considered and may not total 100 percent due to rounding error. Standard errors are reported in the metric of the associated statistic. An asterisk (*) is used in the standard error column to indicate a statistical significant ($p < .05$) difference.

[a] Raw count of students in each category.

[b] Percentages given are based on weighted data, for that subset of individuals who reported post-secondary entry during or before winter 1976-77. Weights for the total comparison group were balanced to reduce differences from Upward Bound participants on specific background variables and on initial grade distribution. The differences existing in this table therefore reflect differential matriculation patterns in the former participant and nonparticipant groups. Weights have been adjusted for indeterminacy of postsecondary entry or Upward Bound participation.

[c] Age categories have been constructed from 1973-74 age categories to reflect age at time of the current study.

[d] Reported value is mean of determinate responses.

[e] Indeterminate category represents missing data.

[f] Standard error not computed, since it would be redundant.

[g] Determined during base-year study.

[h] Standard errors were not computed for differences less than 2 percentage points.

[i] Unweighted and weighted totals within each group, respectively.

Table F.9

AVERAGE HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION

High School Grade Cohort[a]	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
10th Graders	12.66 (N=120)	12.90 (N=59)	12.73 (N=36)	12.74 (N=215)	12.70 (N=184)	c/ 0.12 0.08
11th Graders	13.23 (N=279)	13.19 (N=319)	13.15 (N=112)	13.20 (N=710)	13.06 (N=215)	0.06* 0.06 c/
12th Graders	13.68 (N=364)	13.71 (N=407)	13.61 (N=168)	13.68 (N=939)	13.68 (N=235)	c/ 0.07 c/
Total Group[d]	13.36 (N=763)	13.44 (N=785)	13.36 (N=316)	13.39 (N=1864)	13.35 (N=634)	c/ c/ 0.04*

NOTE: Table entries are based on weighted data and estimate the cell mean highest grade completed as of winter 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Except as otherwise noted, weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables or highest grade completed. Numbers given in parentheses (N=____) show the total number of cases on which the mean was computed.

a/ High school grade in the fall of 1973, at time of initial sample frame definition.

b/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. Standard errors associated with statistically significant (p <.05) differences are indicated by an asterisk (*).

c/ Standard errors were not computed for mean differences less than .05 grade levels.

d/ When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.10

CURRENT ENROLLMENT PERCENTAGES FOR FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION

High School Grade Cohort a/	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference b/
	3 Years	2 Years	1 Year	Total		
10th Graders	83.7% (N=120)	87.3% (N=59)	83.5% (N=36)	84.8% (N=215)	84.8% (N=184)	c/ c/ 8.43
11th Graders	67.1% (N=278)	70.2% (N=314)	52.1% (N=112)	66.3% (N=704)	68.9% (N=215)	5.12 6.23* 4.70
12th Graders	62.0% (N=362)	61.6% (N=403)	67.0% (N=168)	62.7% (N=933)	75.0% (N=237)	5.05* 3.17 c/
Total Group d/	67.2% (N=760)	67.2% (N=776)	63.2% (N=316)	66.8% (N=1852)	75.8% (N=636)	2.40* 3.02 c/

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating enrollment in postsecondary education as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Except as otherwise noted, weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of classifier variables, postsecondary entry, or current enrollment. Numbers given in parentheses (N=__) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

a/ High school grade in the fall of 1973, at time of initial sample frame definition.

b/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

c/ Standard errors were not computed for differences less than 2 percentage points.

d/ When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.11.1

AVERAGE HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE LEVEL OF OFFERING AT THE INSTITUTION THEY LAST ATTENDED

Highest Institutional Offering	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b]	13.10 (N=58)	13.06 (N=83)	12.98 (N=48)	13.05 (N=189)	13.26 (N=105)	0.17 0.11 c/
2 Years or Less	13.09 (N=143)	13.20 (N=176)	13.13 (N=97)	13.14 (N=416)	13.11 (N=220)	c/ c/ 0.06
4 Years	13.46 (N=372)	13.58 (N=366)	13.56 (N=110)	13.52 (N=848)	13.38 (N=168)	0.10 c/ 0.06*
University	13.48 (N=190)	13.61 (N=160)	13.58 (N=61)	13.54 (N=411)	13.63 (N=141)	0.07 c/ 0.08

NOTE: Table values are based on weighted data and estimate the cell mean highest grade completed as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Weights for the comparison group have been balanced to reduce differences from Upward Bound participants on specific background variables and differential initial grade distribution. Weights have been adjusted for indeterminancy of extent of Upward Bound participation, postsecondary entry, or highest grade completed. Numbers given in parentheses (N=___) show the total number of cases on which a given cell mean was computed.

[a] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. Standard errors associated with statistically significant (P<.05) differences are indicated by an asterisk (*).

[b] Indeterminate classification resulted primarily from unavailability of appropriate information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

[c] Standard errors were not computed for mean differences less than .05 grade levels.

Table F.11.2

AVERAGE HIGHEST GRADE COMPLETED, BY INITIAL GRADE COHORT, BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE LEVEL OF OFFERING AT THE INSTITUTION LAST ATTENDED

High School Grade Cohort[a]	Highest Institutional Offering	Upward Bound Participants	Comparison Group (Nonparticipants)
10th graders	Indeterminate[b]	12.84 (N=18)	12.92 (N=25)
	2 Years or less	12.77 (N=54)	12.66 (N=62)
	4 Years	12.73 (N=93)	12.61 (N=55)
	University	12.69 (N=50)	12.74 (N=42)
11th graders	Indeterminate[b]	12.94 (N=79)	12.76 (N=42)
	2 Years or Less	13.03 (N=170)	12.96 (N=84)
	4 Years	13.31 (N=307)	13.19 (N=51)
	University	13.31 (N=154)	13.25 (N=38)
12th graders	Indeterminate[b]	13.21 (N=92)	13.83 (N=38)
	2 Years or Less	13.34 (N=192)	13.30 (N=74)
	4 Years	13.83 (N=448)	13.74 (N=62)
	University	13.87 (N=207)	13.95 (N=61)

NOTE: Table values are based on weighted data and estimate the cell mean highest grade completed as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Weights for the comparison group have been balanced within grade to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminancy of extent of Upward Bound participation, postsecondary entry, or highest grade completed. Numbers given in parentheses (N=__) show the total number of cases on which a given cell mean was computed. Standard errors were not computed for these values due to the relatively small cell sizes involved.

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] Indeterminate classification resulted primarily from unavailability of appropriate information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

Table F.12.1

CURRENT ENROLLMENT PERCENTAGES FOR FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE LEVEL OF OFFERING AT THE INSTITUTION THEY LAST ATTENDED

Highest Institutional Offering	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b]	51.4% (N=61)	51.1% (N=81)	40.8% (N=50)	49.1% (N=192)	47.7% (N=108)	c/ 8.96 c/
2 Years or Less	52.5% (N=141)	59.9% (N=174)	53.6% (N=96)	55.8% (N=411)	66.7% (N=219)	5.08* 8.49 6.07
4 Years	72.7% (N=369)	69.6% (N=365)	71.1% (N=109)	71.1% (N=843)	77.1% (N=168)	5.62 c/ 5.21
University	74.1% (N=189)	80.4% (N=156)	78.8% (N=61)	77.2% (N=406)	93.8% (N=141)	3.27* c/ 5.26

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating enrollment in postsecondary education as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Weights for the comparison group have been balanced to reduce differences from Upward Bound participants on specific background variables and differential initial grade distribution. Weights have been adjusted for indeterminacy of extent of Upward Bound participation, postsecondary entry, or current enrollment. Numbers given in parentheses (N=____) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

a/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. Standard errors associated with statistically significant ($p < .05$) differences are indicated by an asterisk (*).

b/ Indeterminate classification results primarily from unavailability of appropriate information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

c/ Standard errors were not computed for differences less than 2 percentage points.

Table F.12.2

CURRENT ENROLLMENT PERCENTAGES, BY INITIAL GRADE COHORT, FOR FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE LEVEL OF OFFERING AT THE INSTITUTION LAST ATTENDED

High School Grade Cohort[a]	Highest Institutional Offering	Upward Bound Participants	Comparison Group (Nonparticipants)
10th graders	Indeterminate[b]	43.3% (N=18)	46.1% (N=25)
	2 years or less	83.1% (N=54)	93.0% (N=62)
	4 years	94.5% (N=93)	90.6% (N=55)
	University	88.1% (N=50)	83.8% (N=42)
11th graders	Indeterminate[b]	42.6% (N=78)	43.8% (N=42)
	2 years or less	58.7% (N=168)	55.6% (N=84)
	4 years	69.0% (N=307)	79.1% (N=51)
	University	82.7% (N=151)	93.7% (N=38)
12th graders	Indeterminate[b]	54.8% (N=96)	51.7% (N=41)
	2 years or less	46.4% (N=189)	67.0% (N=73)
	4 years	67.6% (N=443)	71.8% (N=62)
	University	71.4% (N=205)	95.1% (N=61)

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating enrollment in postsecondary education as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Weights for the comparison group have been balanced within grade to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminancy of extent of Upward Bound participation, postsecondary entry, or current enrollment. Numbers given in parentheses (N=__) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction). Standard errors were not computed for these values due to the relatively small cell sizes involved.

[a] High school grade in fall of 1973, at time of initial sample frame definition.

[b] Indeterminate classification resulted primarily from unavailability of appropriate information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

Table F.13

AVERAGE HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE UPWARD BOUND HOST STATUS OF THE INSTITUTION THEY LAST ATTENDED

High School Grade Cohort[a]	Institutional Upward Bound Host Status[b]	Former Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
10th Graders	Nonhost	12.76 (N=115)	12.69 (N=133)	0.06
	Host	12.72 (N=100)	12.73 (N=51)	[c]
11th Graders	Nonhost	13.06 (N=373)	13.01 (N=164)	0.07
	Host	13.35 (N=337)	13.15 (N=51)	0.08*
12th Graders	Nonhost	13.56 (N=470)	13.64 (N=172)	0.15
	Host	13.80 (N=469)	13.91 (N=63)	0.14
Total Group[d]	Nonhost	13.27 (N=958)	13.34 (N=469)	0.08
	Host	13.53 (N=906)	13.39 (N=165)	0.07*

NOTE: Table values are based on weighted data and estimate the cell mean highest grade completed as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Except as otherwise indicated, weights for the comparison group have been balanced within grade to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of extent of Upward Bound participation, postsecondary entry, or highest grade completed. Additional indeterminacy of institutional Upward Bound host status involved only nine individuals, and weight adjustment for these cases has not been made. Numbers given in parentheses (N=__) show the total number of cases on which a given cell mean was computed. An asterisk (*) is used in the standard error column to indicate a statistically significant (P<.05) difference.

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] An institutional Upward Bound Host is one sponsoring an Upward Bound Project or having such a project on campus.

[c] Standard errors were not computed for mean differences less than .05 grade levels.

[d] When collapsing over grade cohorts, comparison group weights have been further balanced to correct differences from Upward Bound participants in initial grade distribution.

Table F.14

CURRENT ENROLLMENT PERCENTAGES FOR FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE UPWARD BOUND HOST STATUS OF THE INSTITUTION THEY LAST ATTENDED

High School Grade Cohort[a]	Institutional Upward Bound Host Status[b]	Former Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
10th graders	Nonhost	78.8% (N=115)	85.6% (N=133)	5.78
	Host	93.0% (N=100)	82.3% (N=51)	11.20
11th graders	Nonhost	62.0% (N=369)	62.2% (N=164)	c/
	Host	71.1% (N=334)	82.2% (N=51)	5.09*
12th graders	Nonhost	63.9% (N=464)	73.6% (N=171)	7.07
	Host	61.8% (N=464)	86.0% (N=63)	6.25*
Total group[d]	Nonhost	65.6% (N=948)	71.1% (N=468)	3.43
	Host	68.2% (N=898)	83.6% (N=165)	4.23*

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating enrollment in postsecondary education as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Except as otherwise indicated, weights for the comparison group have been balanced within grade to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminancy of extent of Upward Bound participation, postsecondary entry, or current enrollment. Additional indeterminacy of institutional Upward Bound host status involved only nine individuals, and weight adjustment for these cases has not been made. Numbers given in parentheses (N=__) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction). All standard errors are reported in the percentage metric. An asterisk (*) is used in the standard error column to indicate a statistically significant ($p < .05$) difference.

a/ High school grade in fall of 1973, at time of initial sample frame definition.

b/ An institutional Upward Bound host is one sponsoring an Upward Bound project or having such a project on campus.

c/ Standard errors were not computed for differences less than 2 percentage points.

d/ When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.15

AVERAGE HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANT AND NONPARTICIANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE SPECIAL SERVICES HOST STATUS OF THE INSTITUTION THEY LAST ATTENDED

High School Grade Cohort[a]	Institutional Special Services Host Status[b]	Former Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
10th Graders	Nonhost	12.75 (N=132)	12.69 (N=125)	0.04
	Host	12.74 (N=83)	12.71 (N=59)	[c]
11th Graders	Nonhost	13.13 (N=423)	12.99 (N=150)	0.07*
	Host	13.39 (N=287)	13.15 (N=65)	0.07*
12th Graders	Nonhost	13.59 (N=526)	13.69 (N=161)	0.16
	Host	13.78 (N=413)	13.67 (N=74)	0.11
Total Group[d]	Nonhost	13.30 (N=1081)	13.35 (N=436)	0.08
	Host	13.52 (N=783)	13.36 (N=198)	0.07*

NOTE: Special services refers to Special Services to Disadvantaged Students programs which together with Upward Bound and Talent Search comprise the TRIO Programs. Table values are based on weighted data and estimate the cell mean highest grade completed as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Except as otherwise indicated, weights for the comparison group have been balanced within grade to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminacy of extent of Upward Bound participation, postsecondary entry, or highest grade completed. Additional indeterminacy of institutional Special Services host status involved only nine individuals, and weight adjustment for these cases has not been made. Numbers given in parentheses (N=__) show the total number of cases on which a given cell mean was computed. An asterisk (*) is used in the standard error column to indicate a statistically significant ($P<.05$) difference).

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] An institutional Special Services host is one having such a program on campus.

[c] Standard errors were not computed for mean differences less than .05 grade levels.

[d] When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.16

CURRENT ENROLLMENT PERCENTAGES FOR FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THE SPECIAL SERVICES HOST STATUS OF THE INSTITUTION THEY LAST ATTENDED

High School Grade Cohort a/	Special Services Host Status b/	Former Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Error of Differences
10th graders	Nonhost	80.2% (N=132)	84.9% (N=125)	5.40
	Host	94.5% (N=83)	84.6% (N=59)	7.46
11th graders	Nonhost	62.1% (N=417)	67.5% (N=150)	7.51
	Host	72.2% (N=286)	71.1% (N=65)	c/
12th graders	Nonhost	61.5% (N=520)	74.6% (N=160)	7.19
	Host	64.5% (N=408)	77.7% (N=74)	5.57*
Total group d/	Nonhost	64.2% (N=1069)	72.9% (N=435)	4.00*
	Host	69.9% (N=777)	77.5 (N=198)	3.88*

NOTE: Special services refers to Special Services to Disadvantaged Students programs, which together with Upward Bound and Talent Search comprise the TRIO Programs. Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating enrollment in postsecondary education as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Except as otherwise indicated, weights for the comparison group have been balanced within grade to reduce differences from Upward Bound participants on specific background variables. Weights have been adjusted for indeterminancy of extent of Upward Bound participation, postsecondary entry, or current enrollment. Additional indeterminancy of institutional Special Services host status involved only nine individuals, and weight adjustment for these cases has not been made. Numbers given in parentheses (N=__) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction). All standard errors are reported in the percentage metric. An asterisk (*) is used in the standard error column to indicate a statistically significant (p <.05) difference.

a/ High school grade in fall of 1973, at time of initial sample frame definition.

b/ An institutional Special Services host is one having such a project on campus.

c/ Standard errors were not computed for differences less than 2 percentage points.

d/ When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.17

AVERAGE HIGHEST GRADE COMPLETED BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THEIR PERCEPTION OF INADEQUATE FINANCIAL AID AS AN EDUCATIONAL DETERRENT

Perception of Financial Aid as an Educational Deterrent	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Differences[a/]
	3 Years	2 Years	1 Year	Total		
Not Perceived as Problem	13.31 (N=505)	13.44 (N=541)	13.35 (N=220)	13.37 (N=1266)	13.37 (N=484)	b/ 0.06 0.05*
Perceived as Problem	13.46 (N=257)	13.44 (N=239)	13.41 (N=93)	13.45 (N=589)	13.28 (N=149)	0.08* b/ b/

NOTE: Table values are based on weighted data and estimate the cell mean highest grade completed as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Weights for the comparison group have been adjusted for indeterminacy of extent of Upward Bound participation, postsecondary entry, or highest grade completed. Additional indeterminacy of perception of Financial Aid as an educational deterrent involved only ten individuals, and weight adjustments for these cases have not been made. Numbers given in parentheses (N=___) show the total number of cases on which a given cell mean was computed.

[a/] Three standard errors are reported, one for each of three independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. Standard errors associated with statistically significant (p<.05) differences are indicated by an asterisk (*).

[b/] Standard errors were not computed for mean differences less than .05 grade levels.

Table F.18

CURRENT ENROLLMENT PERCENTAGES FOR FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ENTERED POSTSECONDARY EDUCATION, AS A FUNCTION OF THEIR PERCEPTION OF INADEQUATE FINANCIAL AID AS AN EDUCATIONAL DETERRENT

Perception of Financial Aid As An Educational Deterrent	Extent of Upward Bound Participation				Comparison Group	Standard Errors of Difference a/
	3 Years	2 Years	1 Year	Total	(Nonparticipants)	
Not Perceived as Problem	69.9% (N=501)	66.1% (N=533)	60.5% (N=221)	66.6% (N=1255)	72.3% (N=485)	3.19 3.85 3.93
Perceived as Problem	62.0% (N=258)	71.7% (N=239)	73.3% (N=93)	67.7% (N=590)	80.3% (N=150)	4.38* 7.01 5.59

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating enrollment in postsecondary education as of February-April 1977. These values were computed for the subset of former Upward Bound participants and nonparticipants who indicated they had enrolled in postsecondary education. Weights for the comparison group have been balanced to reduce differences from Upward Bound participants on specific background variables and differential initial grade distribution. Weights for the comparison group have been adjusted for indeterminacy of extent of Upward Bound participation, postsecondary entry, or current enrollment. Additional indeterminacy of perception of Financial Aid as an educational deterrent involved only eight individuals, and weight adjustments for these cases have not been made. Numbers given in parentheses (N=___) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

a/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. Standard errors associated with statistically significant (p <.05) differences are indicated by an asterisk (*).

Table F.19

MEASURES OF ASSOCIATION BETWEEN THE POSTSECONDARY INSTITUTIONAL INDICES

	Student Body Family Income	Institutional Selectivity	Average Cost Per Student	Size of Student Body	Percentage of Student Body Receiving Federal Aid	Average Per-Student Aid	Predominant Student Body Ethnicity	Institutional Control	Highest Offering	Upward Bound Host Status
Institutional Selectivity	38 (51) 17 (23)									
Average Cost Per Student	58 (39) 27 (12)	36 (43) 16 (24)								
Size of Student Body	28 (22) 04 (02)	26 (20) 34 (07)	23 (17) 09 (08)							
Percentage of Student Body Receiving Federal Aid	61 (29) 33 (16)	26 (28) 12 (15)	50 (11) 25 (06)	30 (26) 06 (03)						
Average Per-Student Aid	20 (19) 04 (04)	29 (24) 04 (05)	26 (25) 11 (08)	31 (13) 09 (02)	30 (23) 10 (09)					
Predominant Student Body Ethnicity	39 (56) 11 (13)	45 (53) 07 (08)	21 (15) 02 (00)	37 (19) 05 (00)	51 (46) 12 (11)	48 (16) 12 (00)				
Institutional Control	20 (20) 09 (10)	29 (27) 08 (10)	41 (47) 26 (31)	39 (45) 09 (08)	39 (50) 13 (16)	46 (43) 15 (10)	64 (14) 19 (04)			
Highest Offering	22 (25) 05 (05)	39 (30) 21 (07)	28 (32) 12 (10)	41 (46) 17 (16)	25 (30) 10 (10)	33 (32) 15 (05)	39 (24) 12 (00)	45 (36) 15 (13)		
Upward Bound Host Status	21 (19) 03 (03)	34 (18) 06 (04)	20 (12) 01 (00)	27 (27) 00 (00)	30 (29) 03 (04)	28 (25) 00 (00)	36 (33) 11 (13)	17 (15) 00 (00)	37 (37) 00 (00	
Special Serv. Host Status	17 (17) 01 (01)	21 (19) 02 (03)	21 (21) 01 (01)	29 (29) 01 (01)	19 (19) 01 (01)	18 (11) 00 (00)	33 (29) 08 (10)	27 (25) 00 (00)	24 (24) 00 (00)	44 (44) 19 (19)

NOTE: Associational values presented in this table are based on unweighted two-way contingency tables constructed for the 676 unique postsecondary institutions attended by the student sample. Decimal points implicitly preceding each of the reported values are omitted. Specific categories of the institutional indices are described in detail in Appendix D. Four values are reported in each cell of the table. The uppermost values are Phi Coefficients for two-by-two tables or Cramer indices for larger tables. The lowermost values are symmetric Goodman and Kruskal Lambda statistics, which are close analogues to r^2 for continuous data. Values presented without parentheses were computed using indeterminate data as a separate classification; these values are in all cases based on 676 cases. Values in parentheses were computed for determinate index values only and are based on from 403 to 676 cases depending on the rate of indeterminacy for each pair of variables.

Table F.20

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: FAMILY INCOME OF STUDENTS AT INSTITUTION

Student Body Family Income Characteristics a/	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference b/
	3 Years	2 Years	1 Year	Total		
Indeterminate c/	19.3% (N=151)	22.0% (N=165)	28.3% (N=91)	22.1% (N=407)	36.6% (N=204)	2.63*
Low	30.8% (N=245)	38.9% (N=296)	26.8% (N=96)	33.6% (N=637)	18.8% (N=128)	4.53*
Middle	31.3% (N=242)	23.5% (N=202)	26.4% (N=90)	27.1% (N=534)	27.7% (N=190)	d/
High	18.5% (N=135)	15.6% (N=131)	18.5% (N=48)	17.3% (N=314)	16.9% (N=121)	d/

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

a/ See Appendix D for a more precise definition of this variable.

b/ Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

c/ Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

d/ Standard errors were not calculated for differences less than two percentage points.

Table F.21

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: SELECTIVITY OF INSTITUTION

Institutional Selectivity[a]	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
Indeterminate[c]	31.2% (N=233)	35.5% (N=267)	45.8% (N=152)	35.6% (N=652)	42.7% (N=295)	3.39*
Low	30.1% (N=228)	33.4% (N=262)	20.9% (N=72)	29.9% (N=562)	19.3% (N=112)	4.99*
Moderately Low	12.9% (N=103)	9.7% (N=85)	13.2% (N=36)	11.6% (N=224)	13.3% (N=106)	d/
Moderately High	13.3% (N=116)	13.0% (N=107)	9.8% (N=38)	12.6% (N=261)	16.2% (N=75)	3.88
High	12.4% (N=93)	8.3% (N=73)	10.4% (N=27)	10.3% (N=193)	8.5% (N=55)	d/

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a] See Appendix D for a more precise definition of this variable.

[b] Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant (p <.05) difference.

[c] Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

[d] Standard errors were not calculated for differences less than two percentage points.

Table F.22

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: ANNUAL PER-STUDENT COST AT INSTITUTION

Average Annual Per-Student Cost[a/]	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b/]
	3 Years	2 Years	1 Year	Total		
Indeterminate[c/]	19.8% (N=155)	22.7% (N=171)	30.1% (N=98)	22.8% (N=335)	38.0% (N=217)	2.40*
Less than $2000	16.0% (N=137)	21.4% (N=197)	20.1% (N=70)	19.0% (N-404)	14.7% (N=119)	3.35
$2000 - $2499	19.4% (N=139)	21.8% (N=146)	14.7% (N=54)	19.6% (N=339)	15.1% (N=111)	2.96
$2500 - $2999	23.4% (N=183)	17.3% (N=137)	16.8% (N=55)	19.7% (N=375)	14.8% (N=118)	2.97
$3000 - $3999	14.3% (N=113)	11.4% (N=97)	13.0% (N=32)	12.8% (N=242)	8.8% (N=67)	1.93*
$4000 or more	7.2% (N=46)	5.4% (N=46)	5.3% (N=16)	6.1% (N=108)	8.7% (N=11)	3.21

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a/] See Appendix D for a more precise definition of this variable.

[b/] Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant (p <.05) difference.

[c/] Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

Table F.23

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: STUDENT BODY SIZE AT INSTITUTION

Full-Time Equivalent Undergraduate Enrollment[a/]	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b/]
	3 Years	2 Years	1 Year	Total		
Indeterminate[c/]	11.4% (N=90)	15.8% (N=112)	19.6% (N=66)	14.7% (N=268)	23.3% (N=133)	1.99*
Less than 500	4.8% (N=38)	4.8% (N=42)	6.5% (N=24)	5.1% (N=104)	4.6% (N=51)	d/
500 - 999	10.9% (N=73)	10.4% (N=76)	12.0% (N=41)	10.9% (N=190)	13.0% (N=73)	3.09
1000 - 2999	30.6% (N=229)	35.1% (N=278)	25.7% (N=84)	31.6% (N=591)	24.1% (N=150)	4.65
3000 - 7999	21.8% (N=174)	21.4% (N=170)	23.4% (N=69)	21.9% (N=413)	16.7% (N=118)	4.55
8000 or more	20.6% (N=169)	12.6% (N=116)	12.8% (N=41)	15.8% (N=326)	18.4% (N=118)	2.97

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a/] See Appendix D for a more precise definition of this variable.

[b/] Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant (p <.05) difference.

[c/] Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

[d/] Standard errors were not calculated for differences less than two percentage points.

Table F.24

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: PROPORTION OF STUDENTS RECEIVING FEDERAL AID AT INSTITUTION

Proportion of Students At Institutions Receiving Fed. Aid[a]	Extent of Upward Bound Participation				Comparison Group	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total	(Nonparticipants)	
Indeterminate[c]	17.9% (N=142)	21.7% (N=161)	28.3% (N=91)	21.3% (N=394)	36.4% (N=203)	2.60* 4.35 3.33
Less than .15	26.4% (N=199)	23.7% (N=206)	24.3% (N=84)	24.9% (N=489)	24.7% (N=189)	[d] [d] 2.78
.15 to .29	27.7% (N=218)	21.5% (N=185)	27.5% (N=81)	25.1% (N=484)	18.6% (N=162)	4.59 3.99 3.98
.30 or Greater	28.0% (N=214)	33.1% (N=242)	19.9% (N=69)	28.7% (N=525)	20.3% (N=89)	3.41* 3.31* 2.95

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=___) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a] See Appendix D for a more precise definition of this variable.

[b] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

[c] Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

[d] Standard errors were not calculated for differences less than two percentage points.

Table F.25

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: AVERAGE FINANCIAL AID PER STUDENT AT INSTITUTION

Average Aid Per Student a/	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors Of Difference b/
	3 Years	2 Years	1 Year	Total		
Indeterminate c/	17.2% (N=138)	21.2% (N=160)	26.4% (N=92)	20.5% (N=390)	29.9% (N=176)	2.21* 3.31* 3.03
Less than $50	9.5% (N=69)	11.4% (N=97)	13.6% (N=39)	11.0% (N=205)	10.5% (N=89)	d/ 3.15 d/
$50 to $99	15.5% (N=118)	15.8% (N=138)	12.6% (N=46)	15.1% (N=302)	16.1% (N=123)	d/ 2.63 d/
$100 to $899	48.1% (N=386)	47.8% (N=370)	42.6% (N=135)	47.0% (N=891)	35.1% (N=228)	4.33* 4.64 d/
$900 or more	9.6% (N=62)	3.9% (N=29)	4.9% (N=13)	6.4% (N=104)	8.4% (N=27)	d/ d/ 2.79*

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=___) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

a/ See Appendix D for a more precise definition of this variable.

b/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant (p <.05) difference.

c/ Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

d/ Standard errors were not calculated for differences less than two percentage points.

Table F.26

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: PREDOMINANT STUDENT ETHNICITY AT INSTITUTION

Predominant Student Ethnicity	Extent of Upward Bound Participation				Comparison Group	Standard Errors of Difference[a]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b]	10.7% (N=88)	15.5% (N=121)	22.2% (N=78)	14.8% (N=287)	24.0% (N=142)	2.15*
White	72.4% (N=548)	68.1% (N=531)	68.5% (N=210)	69.9% (N=1279)	67.7% (N=476)	3.63
Black	16.9% (N=137)	16.4% (N=142)	9.2% (N=37)	15.3% (N=316)	8.3% (N=34)	2.84*

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=___) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a] Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant (p <.05) difference).

[b] Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

Table F.27

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: CONTROL OF INSTITUTION

Type of Institutional Control	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b]	8.7% (N=69)	13.0% (N=97)	18.8% (N=66)	12.3% (N=232)	21.0% (N=127)	1.92*
Public	64.6% (N=506)	60.5% (N=493)	60.1% (N=193)	62.1% (N=1192)	59.1% (N=417)	3.37
Private	25.5% (N=188)	25.1% (N=190)	19.5% (N=59)	24.3% (N=437)	18.2% (N=88)	3.55
Proprietary	1.1% (N=10)	1.4% (N=14)	1.7% (N=7)	1.4% (N=31)	1.6% (N=11)	[c]

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=___) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a] Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant (p <.05) difference.

[b] Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

[c] Standard errors were not calculated for differences less than two percentage points.

Table F.28

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: LEVEL OF OFFERING AT INSTITUTION

Level of Offering	Extent of Upward Bound Participation				Comparison Group	Standard Errors of Difference a/
	3 Years	2 Years	1 Year	Total		
Indeterminate[b/]	8.2% (N=64)	12.4% (N=91)	16.2% (N=56)	11.4% (N=211)	18.4% (N=114)	1.81*
Two-Years (or Less)	20.7% (N=145)	22.7% (N=177)	28.4% (N=98)	22.9% (N=420)	31.0% (N=220)	3.39*
Four-Year	47.0% (N=373)	46.5% (N=366)	35.9% (N=110)	44.9% (N=849)	21.6% (N=168)	4.32*
University	24.1% (N=191)	18.4% (N=160)	19.5% (N=61)	20.9% (N=412)	29.0% (N=141)	4.25

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=___) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

a/ Standard errors given are for difference between total UB group and comparison group. Standard errors for differences between various UB exposure groups were not calculated. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

b/ Indeterminate data resulted primarily from unavailability of the classifying information for those postsecondary institutions attended by sample members. A very small proportion of the indeterminate data resulted from questionnaire item nonresponse or internal inconsistency.

Table F.29

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: UPWARD BOUND HOST STATUS OF INSTITUTION

Upward Bound Host Status of Institution	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a/]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b/]	0.8% (N=6)	0.8% (N=8)	3.6% (N=8)	1.3% (N=22)	2.4% (N=9)	c/ c/ c/
Non-host Institution	48.3% (N=357)	51.8% (N=411)	60.8% (N=196)	52.0% (N=964)	74.5% (N=469)	d/ d/ d/
Host Institution	50.9% (N=410)	47.4% (N=375)	35.6% (N=121)	46.8% N=906)	23.1% (N=165)	3.24* 3.74* 3.98

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

a/ Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

b/ Indeterminate data represent item nonresponse or inconsistency.

c/ Standard errors were not calculated for multiple contrasts involving sets of proportions, all of which were less than .05.

d/ Standard errors not computed since they would be redundant.

Table F.30

DISTRIBUTIONS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS AMONG DIFFERENT TYPES OF POSTSECONDARY INSTITUTIONS: SPECIAL SERVICES HOST STATUS OF INSTITUTION

Presence of Special Services Program at Institution	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a/]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b/]	0.8% (N=6)	0.8% (N=8)	3.6% (N=8)	1.3% (N=22)	2.4% (N=9)	c/ c/ c/
Not Host	53.5% (N=417)	61.0% (N=476)	59.1% (N=194)	57.6% (N=1087)	62.3% (N=436)	d/ d/ d/
Host	45.7% (N=350)	38.3% (N=310)	37.3% (N=123)	41.1% (N=783)	35.3% (N=198)	3.61 3.84 3.21*

NOTE: These data are based on the subset of individuals who indicated postsecondary entry; institutional characteristics are those associated with the institution last or currently attended. Special services programs refer to Special Services for Disadvantaged Students programs, which together with Upward Bound and Talent Search comprise the so-called TRIO Programs. Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a/] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

[b/] Indeterminate data represent item nonresponse or inconsistency.

[c/] Standard errors were not calculated for multiple contrasts involving sets of proportions, all of which were less than .05.

[d/] Standard errors not computed since they would be redundant.

Table F.31.1

EMPLOYMENT STATUS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ARE NOT CURRENTLY ENROLLED IN SCHOOL

Current Work Status	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b]	1.0% (N=6)	1.0% (N=5)	2.1% (N=8)	1.3% (N=19)	1.4% (N=10)	c/ c/ c/
Unemployed	39.6% (N=141)	36.2% (N=179)	41.6% (N=132)	38.7% (N=452)	32.4% (N=234)	3.26 3.70 6.11
Working Part-Time	11.8% (N=50)	7.6% (N=40)	5.1% (N=24)	8.2% (N=114)	8.4% (N=57)	c/ 1.73* 2.84
Working Full-Time[d]	47.6% (N=172)	55.2% (N=293)	51.2% (N=172)	51.8% (N=637)	57.7% (N=496)	3.15 c/ 5.35

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Weights in both groups have been adjusted for indeterminate data as to enrollment status in February-April 1977. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

[b] Indeterminate data represent item nonresponse.

[c] Standard errors were not computed for differences less than 2 percentage points.

[d] Includes those in military service.

Table F.31.2

EMPLOYMENT STATUS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS WHO ARE CURRENTLY ENROLLED IN SCHOOL (FULL-TIME OR PART-TIME)

Current Work Status	Extent of Former Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a/]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b/]	1.7% (N=9)	2.2% (N=6)	1.6% (N=4)	1.9% (N=19)	1.3% (N=4)	c/ c/ c/
Unemployed	61.1% (N=342)	60.2% (N=346)	57.1% (N=124)	60.1% (N=812)	56.6% (N=275)	3.30 4.70 c/
Working Part-Time	30.8% (N=157)	29.6% (N=164)	26.5% (N=50)	29.6% (N=371)	31.0% (N=144)	c/ 3.89 c/
Working Full-Time[d/]	6.3% (N=33)	8.0% (N=49)	14.9% (N=32)	8.5% (N=114)	11.1% (N=53)	1.94 2.73* c/

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Weights in both groups have been adjusted for indeterminate data as to enrollment status in February-April 1977. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a/] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

[b/] Indeterminate data represent item nonresponse.

[c/] Standard errors were not computed for differences less than 2 percentage points.

[d/] Includes those in military service.

Table F.32

CURRENT MILITARY SERVICE STATUS OF FORMER UPWARD BOUND PARTICIPANTS AN NONPARTICIPANTS

Military Service Status	Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
Indeterminate[a/]	5.8% (N=137)	1.6% (N=23)	0.95*
Never in Service	82.9% (N=2266)	86.9% (N=1145)	1.56*
Now in Service	8.5% (N=208)	9.0% (N=106)	b/
Veteran	2.8% (N=64)	2.5% (N=42)	b/

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differentiatial initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction). All standard errors are reported in the percentage metric. An asterisk (*) in the standard error column is used to indicate a statistically significant (p <.05) difference.

a/ Indeterminate data represent item nonresponse.

b/ Standard errors were not computed for differences less than 2 percentage points.

Table F.33

EDUCATIONAL EXPECTATION OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

Highest Education Expected	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[a]
	3 Years	2 Years	1 Year	Total		
Indeterminate[b]	8.3% (N=66)	9.5% (N=90)	15.4% (N=92)	10.4% (N=248)	5.1% (N=63)	1.35* 2.72* [c]
High School Only	3.3% (N=33)	8.9% (N=96)	14.6% (N=85)	8.2% (N=214)	16.5% (N=282)	1.86* 1.73* 1.20*
Vocational, Technical, or Business School	10.4% (N=92)	14.7% (N=160)	16.5% (N-98)	13.6% (N=350)	22.9% (N=264)	2.67* 1.83* 2.16*
Some College	14.3% (N=131)	10.6% (N=110)	14.4% (N=87)	12.8% (N=328)	15.5% (N=191)	2.01 1.88 2.05
Complete four-year Program	35.3% (N=346)	33.7% (N=376)	25.9% (N=153)	32.5% (N=875)	30.0% (N=328)	3.63 3.03* [c]
Complete Graduate Program	28.4% (N=272)	22.5% (N=288)	13.2% (N=81)	22.4% (N=641)	10.0% (N=149)	1.35* 1.87* 2.66*

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a] Three standard errors are reported, one for each of three independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant ($p < .05$) difference.

[b] Indeterminate data represent item nonresponse and internally inconsistent responses.

[c] Standard errors were not computed for differences less than 2 percentage points.

Table F.34

EXTENT TO WHICH POOR GRADES WERE SEEN TO BE EDUCATIONAL DETERRENTS BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

	Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
Indeterminate[a]	1.8% (N=41)	2.0% (N=27)	b/
Was not a deterrent	86.4% (N=2330)	87.8% (N=1160)	b/
Was a deterrent	11.8% (N=304)	10.2% (N=129)	b/

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

a/ Indeterminate data represent item nonresponse.

b/ Standard errors were not calculated for differences less than two percentage points.

Table F.35

EXTENT TO WHICH POOR HIGH SCHOOL PREPARATION WAS SEEN TO BE AN EDUCATIONAL DETERRENT BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

	Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
Indeterminate[a]	1.8% (N=41)	2.0% (N=27)	b/
Was not a deterrent	79.4% (N=2093)	83.2% (N=1066)	c/
Was a deterrent	18.8% (N=541)	14.8% (N=223)	1.73*

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction). All standard errors are reported in the percentage metric. An asterisk (*) in the standard error column is used to indicate a statistically significant (p <.05) difference.

a/ Indeterminate data represent item nonresponse.

b/ Standard errors were not calculated for differences less than two percentage points.

c/ Standard error not computed since it would be redundant information.

Table F.36

EXTENT TO WHICH UNAVAILABILITY OF DESIRED PROGRAM WAS SEEN TO BE AN EDUCATIONAL DETERRENT BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIANTS

	Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
Indeterminate[a/]	1.8% (N=41)	2.0% (N=27)	b/
Was not a deterrent	91.8% (N=2446)	90.6% (N=1170)	b/
Was a deterrent	6.5% (N=188)	7.4% (N=119)	b/

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction).

[a/] Indeterminate data represent item nonresponse.

[b/] Standard errors were not calculated for differences less than two percentage points.

Table F.37

EXTENT TO WHICH FAMILY RESPONSIBILITIES WERE SEEN TO BE EDUCATIONAL DETERRENTS BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

	Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
Indeterminate[a]	1.8% (N=41)	2.0% (N=27)	b/
Was not a deterrent	81.1% (N=2153)	77.5% (N=1087)	c/
Was a deterrent	17.1% (N=481)	20.5% (N=202)	2.05

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction). All standard errors are reported in the percentage metric. Differences shown were not statistically significant at the p <.05 level.

a/ Indeterminate data represent item nonresponse.

b/ Standard errors were not calculated for differences less than two percentage points.

c/ Standard error not computed since it would be redundant information.

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Table F.38

EXTENT TO WHICH PERSONAL AND FAMILY FINANCES WERE SEEN TO BE AN EDUCATIONAL DETERRENT BY FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

	Upward Bound Participants	Comparison Group (Nonparticipants)	Standard Errors of Difference
Indeterminate[a/]	1.8% (N=41)	2.0% (N=27)	b/
Was not a deterrent	60.5% (N=1555)	70.2% (N=910)	c/
Was a deterrent	37.7% (N=1079)	27.8% (N=379)	3.15*

NOTE: Percentages given are column percentages based on weighted data. Weights for the comparison group have been balanced to reduce differences from the Upward Bound participants on specific background variables and differential initial grade distribution. Numbers given in parentheses (N=__) show the number of cases in the associated cell (i.e., the number of cases contributing to the numerator of the fraction). All standard errors are reported in the percentage metric. An asterisk (*) is used in the standard error column to indicate a statistically significant (p <.05) difference.

[a/] Indeterminate data represent item nonresponse.

[b/] Standard errors were not calculated for differences less than two percentage points.

[c/] Standard error not computed since it would be redundant information.

Table F.39

PERCENTAGES OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS INDICATING THAT INADEQUATE FINANCIAL AID WAS A DETERRENT IN OBTAINING THEIR DESIRED LEVEL OF EDUCATION

High School Grade Cohort[a]	Extent of Upward Bound Participation				Comparison Group (Nonparticipants)	Standard Errors of Difference[b]
	3 Years	2 Years	1 Year	Total		
10th Graders	23.9% (N=163)	24.4% (N=110)	16.4% (N=105)	22.7% (N=378)	19.0% (N=424)	3.42 5.18 c/
11th Graders	30.7% (N=340)	24.8% (N=476)	21.1% (N=234)	26.2% (N=1050)	16.9% (N=430)	3.05* 4.14 3.16
12th Graders	34.7% (N=437)	27.8% (N=534)	25.2% (N=257)	30.0% (N=1228)	17.4% (N=423)	2.94* 4.27 4.07
Total Group[d]	31.4% (N=940)	26.3% (N=1120)	22.1% (N=596)	27.5% (N=2656)	17.8% (N=1277)	1.98* 2.71* 2.78

NOTE: Percentages given are based on weighted data and estimate the relative number of individuals in each cell indicating that inadequate financial aid was an educational deterrent. Indeterminate data rates were less than 2.5% in every cell with no indication of systematic between-cell differences; thus, no weight adjustments for indeterminate data were attempted. Except as otherwise noted, weights for the comparison group have been balanced, within grade, to reduce differences from Upward Bound participants on specific background variables. Numbers given in parentheses (N=__) show the total number of cases on which a given cell percentage was computed (i.e., the number of cases contributing to the denominator of the fraction).

[a] High school grade in the fall of 1973, at time of initial sample frame definition.

[b] Three standard errors are reported, one for each of 3 independent contrasts within each row. Specifically the standard errors are associated with contrasts between (1) total Upward Bound group (vs.) comparison group, (2) 1-year participants (vs.) greater than 1-year participants, and (3) 2-year participants (vs.) 3-year participants, from top to bottom, respectively. All standard errors are reported in the percentage metric. An asterisk (*) is used to indicate a statistically significant difference.

[c] Standard errors were not computed for differences less than 2 percentage points.

[d] When collapsing over grade cohorts, comparison group weights have been further balanced to correct for differences from Upward Bound participants in initial grade distribution.

Table F.40

DEMOGRAPHIC CHARACTERISTICS OF FORMER UPWARD BOUND PARTICIPANTS AND NONPARTICIPANTS

Variable	Classification	Former Upward Bound Participants		Comparison Group (Nonparticipants)		Standard Error of Difference
		Number of Cases[a/]	Percentage[b/]	Number of Cases[a/]	Percentage[b/]	
Age[c/]	17 or less	26	0.9%	15	1.1%	
	18	180	6.9%	179	14.4%	
	19	632	23.0%	384	31.8%	
	20	963	35.4%	404	28.3%	
	21	676	26.1%	262	20.7%	
	22	147	5.8%	52	2.4%	
	23	26	1.1%	6	0.2%	
	24 or more	5	0.2%	2	0.1%	
	Average	2655	20.02[d/]	1304	19.62[d/]	0.09*
	Indeterminate[e/]	20	0.7%	12	1.0%	
Race	Black	1675	62.0%	452	26.6%	4.21*
	White	468	17.3%	623	57.3%	2.40*
	Other	504	19.8%	196	12.3%	2.32*
	Indeterminate[e/]	28	1.0%	45	3.8%	0.75*
Sex	Male	1169	44.6%	687	51.3%	2.54*
	Female	1506	55.4%	626	48.3%	f/
	Indeterminate[e/]	0	0.0%	3	0.4%	g/
Grade Level (Fall 73)	10th grade	383	14.7%	436	36.2%	4.56*
	11th grade	1060	38.4%	443	32.6%	2.70*
	12th grade	1232	46.9%	437	31.2%	3.28*
Poverty Status[h/]	Poverty level	1738	64.0%	619	34.2%	2.41*
	Not poverty level	613	23.0%	666	62.3%	2.85*
	Indeterminate[e/]	324	13.0%	31	3.5%	1.38*
Academic Risk Status[h/]	Risk	1180	47.1%	632	46.3%	g/
	Not risk	1477	52.3%	678	53.0%	g/
	Indeterminate[e/]	18	0.5%	6	0.7%	g/
Total Cases[i/]		2675	20,592	1316	1,231,788	

NOTE: Percentages are computed within student groups for the particular variable considered and may not total 100 percent due to rounding error. Standard errors are reported in the metric of the associated statistic. An asterisk (*) is used in the standard error column to indicate a statistically significant (p <.05) difference.

[a/] Raw count of students in each category.

[b/] Percentages given are based on unbalanced data; however, weight adjustments for study eligibility and for telephone subsampling have been made.

[c/] Age categories have been constructed from 1973-74 age categories to reflect age at time of the current study.

[d/] Reported value is mean of determinate responses.

[e/] Indeterminate category represents missing data.

[f/] Standard error not computed, since it would be redundant.

[g/] Standard errors were not computed for differences less than 2 percentage points.

[h/] Determined during base year study.

[i/] Unweighted and weighted totals within each group, respectively.

Table F.41

DEMOGRAPHIC CHARACTERISTICS OF FORMER UPWARD BOUND PARTICIPANTS BY LENGTH OF PARTICIPATION

Variable	Classification	Extent of Participation									
		3 Years		2 Years		Standard Errors of Difference[c]	Total (Greater Than 1 Year)		1 Year		Standard Errors of Difference[d]
		Number of Cases[a]	Percentage[b]	Number of Cases[a]	Percentage[b]		Number of Cases[a]	Percentage[b]	Number of Cases[a]	Percentage[b]	
Age[e]	17 or less	15	1.8%	4	0.2%		19	0.9%	6	0.7%	
	18	92	9.5%	57	5.9%		149	7.5%	30	6.2%	
	19	230	24.6%	270	22.7%		500	23.6%	125	19.8%	
	20	328	34.2%	416	36.8%		744	35.6%	215	34.8%	
	21	220	23.1%	293	26.8%		513	25.1%	158	29.0%	
	22	43	5.5%	58	5.4%		101	5.5%	45	6.9%	
	23	7	0.9%	12	1.3%		19	1.1%	7	0.9%	
	24 or more	0	0.0%	4	0.3%		4	0.2%	1	0.1%	
	Average	935	16.88[f]	1114	17.08[f]	0.08*	2049	16.99[f]	587	17.12[f]	0.05*
	Indeterminate[g]	5	0.4%	6	0.5%		11	0.4%	9	1.6%	
Race	Black	637	69.0%	721	63.0%	0.84*	1358	65.7%	306	50.5%	1.12*
	White	132	12.8%	189	17.1%	1.05*	321	15.2%	142	24.5%	1.42*
	Other	160	17.3%	203	19.4%	0.98*	363	18.4%	138	23.3%	1.24
	Indeterminate[g]	11	1.0%	7	0.5%	[h]	18	0.7%	10	1.7%	[h]
Sex	Male	421	45.8%	467	43.6%	1.71	888	44.6%	271	45.7%	[h]
	Female	519	54.2%	653	56.4%	[i]	1172	55.4%	325	54.3%	[h]
Grade Level (Fall 1973)	10th Grade	163	17.9%	110	10.7%	1.58*	273	13.9%	105	17.4%	1.59*
	11th Grade	340	34.7%	476	41.5%	0.87*	816	38.4%	234	38.4%	[h]
	12th Grade	437	47.5%	534	47.9%	[h]	971	47.7%	257	44.2%	1.50*
Poverty Status[j]	Poverty level	597	63.0%	743	65.2%	1.68	1340	64.2%	385	62.9%	[h]
	Not poverty level	227	23.5%	249	22.2%	[h]	476	22.8%	134	23.4%	[h]
	Indeterminate[g]	116	13.4%	128	12.6%	[h]	244	13.0%	77	13.7%	[h]
Academic Risk Status[j]	Risk	392	43.1%	492	48.8%	1.27*	884	46.2%	283	49.5%	1.54*
	Not risk	539	56.2%	621	50.7%	1.37*	1160	53.2%	311	50.3%	1.61
	Indeterminate[g]	9	0.7%	7	0.6%	[h]	16	0.6%	2	0.2%	[h]
Total Cases[k]		940	7185	1120	8713		2060	15898	596	4694	

NOTE: Percentages are computed within participation groups for the particular variable considered and may not total 100 percent due to rounding error. Standard errors are reported in the metric of the associated statistic. An asterisk (*) is used in the standard error columns to indicate a statistical significant (p <.05) difference.

[a] Raw count of students in each category.

[b] Percentages given are based on weighted data; weight adjustments for study eligibility, telephone subsample, and indeterminacy of extent of participation have been made.

[c] Related difference is that between 2-year participants and 3-year participants.

[d] Related difference is that between 1-year participants and greater-than-1-year participants.

[e] Age categories have been constructed from 1973-74 age categories to reflect age at time of the current study.

[f] Reported value is mean of determinate responses.

[g] Indeterminate category represents missing data.

[h] Standard errors were not computed for differences less than 2 percentage points.

[i] Standard error not computed, since it would be redundant.

[j] Determined during base-year study.

[k] Unweighted and weighted totals within groups.

Appendix G

Procedures and Results of Relational Analyses

Appendix G

Procedures and Results of Relational Analyses

I. GENERAL

This appendix addresses the relational analyses leading to the findings reported in Chapter 5 of this report. Specifically emphasized are: (a) the general analytic approach and rationale, (b) constraints and limitations on the analyses, (c) specific approaches to model building and testing, (d) specific analytic procedures employed, and (e) supporting tabular results. Topics that are closely related to these analyses are detailed elsewhere: weighting adjustments, in Appendix E; data analysis and analysis variable construction in Appendix D.

Although the comparative descriptive analyses, described in Appendix F and reported in Chapter 4, address important questions of national impact of the Upward Bound (UB) program, there are other obviously important questions concerning the program and its possible impact at a less global level. Specifically, it was considered important to determine the extent to which educational outcomes were a function of individual background characteristics (e.g., age, sex, race, poverty status, academic risk status), initial grade level, extent of UB program participation, and specific UB project association.[1] Some such factors were effectively adjusted from the results of the comparative descriptive analyses, and thus any unique relationships of these concommitant variables with extent of UB participation (either singly or in interaction with one another) were not assessed. One of the variables, specific UB project association, was not considered in the comparative descriptive analyses other than in the standard error computations (see Appendix E, Section IV). The relationship of this variable in interaction with extent of UB participation to the various outcome measures is theoretically quite important, since such an interactive relationship reflects differential project effectiveness.

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[1] Although nonparticipants were not members of a UB project, by definition, they were associated with a project through their association with one of the project "feeder schools" (i.e., high schools from which the UB project recruited and within which UB participants from that project were enrolled). As such, project association is seen to represent an important contextual variable. Moreover, any interaction of this contextual variable with extent of UB participation signals differential project effectiveness.

The relational analyses were planned and specified jointly by RTI and OPBE; they were conducted for only a subset of the study outcome measures, specifically highest grade completed, entry into postsecondary education (PSE), and educational expectations. The specific questions addressed by the analyses were:

1. To what extent and in what manner are individual characteristics (ethnicity, sex, poverty status, academic risk status, age), initial educational starting point (grade cohort), and extent of Upward Bound participation jointly related to educational expectations, persistence, and progress?
2. To what extent and in what manner is context (as reflected in specific project association) related to educational expectations, persistence, and progress, when considered jointly with individual background factors, extent of Upward Bound participation, and initial educational starting point?
3. Are these relationships relatively stable within the various categories of individual classification? (i.e., Do the variables interact in their relationship to the educational outcomes considered?)

In conducting the relational analyses, the unit of analysis was the individual sample member. A general linear model approach was employed which included as predictor terms only "main effect" terms (e.g., those involving the variables specified above considered singly) and first-order interaction effect terms (e.g., those involving the specified variables considered in pairwise combinations). Higher order interaction terms were not used since they would have introduced problems of parameter proliferation and possibly those of design matrix indeterminacy. Further, existence of the first order interactions, rather than precision of estimates of their magnitude, was the major concern.

The basic approach used in analysis was multiple regression, a very general statistical technique whereby the relationship of one variable to a *set* of other variables may be assessed.[2] Generally, the technique allows the determination of that *linear combination* of those variables in the predictor set which best predicts (shows the greatest zero-order correlation with) the single criterion variable. The important conceptual difference between multiple regression and simple bivariate regression is that of partial correlation. Specifically, in determining the best linear combination of the predictor variables, it is important to go beyond the simple zero-order relationship of each individual predictor to the criterion. Rather, one is concerned with evaluating the relationship of a predictor variable to the *residual* portion of the criterion variable that remains when the prediction of all other variables in the predictor set has been removed (partialed). In addition to the solution for the necessary weights to produce the most predictive linear combination of the predictor variables, the analysis yields a statistic, $\underline{R}^2$ (the square of the multiple correlation coefficient), which is a measure of the proportion of variability of the criterion variable which may be explained by all the predictor variables considered *simultaneously*.

In addressing the analysis questions posed above, the goal of the analyses was to produce from the available predictor variable set (i.e., the student characteristics, initial grade level, extent of UB participation, specific project membership--together with associated first-order interactions) the most parsimonious linear model explaining variation in the various criterion measures (i.e., highest grade completed, postsecondary entry, educational expectations). The logical steps in the analyses thus involved addressing the analysis questions in the reverse order of their presentation.

The attempts to produce a parsimoneous predictive model avoided the commonly used forward stepwise approach (i.e., adding one variable, or set of variables, at a time to the prediction model on the basis of one of several possible decision rules). The forward stepwise approach tends to make the most of chance variation and covariation among certain variables and to have a tendency of producing spurious results and oversimplified or nonrational models. Rather, the approach used was a rational stepdown elimination approach, whereby the full prediction model (i.e., the model containing all predictor variables of interest, including interaction terms)[3] was reduced according to a predetermined set of steps and rules. At each step of the reduction, the reduced model was tested against the previous model, by both statistical and magnitude tests, to determine if the reduction in the proportion of variance explained (as a result removing a particular set of terms from the model) was significant. The sequential process ended when no additional terms (or set of terms) could be removed from the model without significantly reducing R^2 (see Section III below for detail).

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[2] See, for example, Draper, N. R. and Smith, H. *Applied Regression Analysis*. New York: Wiley, 1966.

The choice of variables in the predictor variable set was, of course, limited by the data available from the follow-up and base-year studies. The choice of the three specific criterion variables was also limited in that respect and was further constrained by other considerations. One of these constraints was cost; these analyses were relatively expensive, and the choice of analysis variables reflected major areas of interest rather than peripheral ones. Moreover, some outcome variables were not suited to these analyses since they would be restricted to relatively small subsets of individuals (e.g., analysis of postsecondary persistence could have been conducted only for those who entered PSE). It was considered dangerous to build complex models (i.e., large number of parameters) on the basis of the relatively small number of analysis units available in these subsets.

As previously implied, different regression models were built for each of the three criterion variables used. Such an approach is not uniformly acceptable as the most appropriate for cases such as this, since it does not take into account relationships between the criterion variables themselves. Another approach that is available is canonical analysis, which is multivariate in the criterion variable set as well as in the predictor variable set. While the appropriateness of the use of canonical anaysis is an open question, the interpretability of results from such analyses typically is not. In such analyses, the various criterion variables are also combined in a linear fashion into a "most predictable variable," which is often difficult to interpret in respect to the original variables themselves. At the risk of sacrificing some degree of appropriateness, RTI opted for the more interpretable approach for this study.

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[3] Although the full model was highly parameterized and somewhat clumsy as a predictive system, there were sufficient degrees of freedom, given the use of individual sample members as the unit of analysis.

The relational analyses used, where feasible, sampling weights and procedures appropriate for weighted regression within the multistage probability sample design of this study. Since the regression procedures employed demand complete data on all variables in the model, the problem of indeterminate data had been anticipated as one that would require imputation techniques. The rate of data indeterminacy was not great, however, and rather than resorting to imputation schemes, the more conservative weighting class adjustment approach was implemented. This approach involves apportioning the sample weights of those with data indeterminacy to others most like them with full data availability (see Appendix E, Section V, for additional detail).

Aside from the various theoretical and philosophical issues involving the validity of inferences from surveys, the basic problem of producing unbiased estimates of regression parameters and estimates of the associated standard errors has been a particularly thorny issue in dealing with results from stratified multistage sample designs such as the one used here. Most of the available statistical software packages treat the sample as independent random observations, ignoring the sample design. This approach is convenient, but it is inappropriate, since it does not account for unequal probabilities of selection. The application of sampling weights is possible through some software packages, allowing correct estimates of regression coefficients, but appropriate error variance estimates typically are not produced. In fact, it is not possible to obtain explicit expressions for variance estimates of complex estimators such as regression coefficients within complex survey sample designs; however, there are various approximation procedures available. While there is no single "best" way to approach the matter, RTI has developed an approach that is considered optimal or near optimal in most circumstances. The approach combines the use of the Horvitz-Thompson estimator[4] and Taylorized deviation[5] which have been incorporated into a statistical software routine available at RTI.[6]

Although the Horvitz-Thompson estimator is not the "best" estimator in all cases, neither is any other estimator. Moreover, the Horvitz-Thompson estimator is widely used and has been found to be an admissible estimate. The Taylorized deviations approach is also not necessarily the "best" approximation procedure; but it does possess some practical and computational advantages over other available techniques (e.g., independent replications, pseudo-replications, or jackknife techniques). The Taylorized approach is, however, less known and is detailed in Subsection IV.B of this appendix. While the procedure is an appropriate one, it imposes certain additional restrictions on analysis, particularly in regard to the degrees of freedom (and thus the number of parameters) available to the regression model and in regard to the use of variables in the model that were used in defining the original sampling strata. Specifically, models involving specific project association (a clustering factor in the original sample design) or more than 25 regression degrees of freedom could not be analyzed by this procedure.

II. SOME CONSTRAINTS AND LIMITATIONS

In adopting the regression model, certain assumptions that are associated with the model were also tacitly adopted. The regression model is best suited for continuous variables and for detecting linear relationships; however, it is widely used with categorical data--reparameterized to dummy variables--in the predictor variable set and can be modified by appropriate polynomial terms to detect other than linear relationships among the variables.

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[4] Horvitz, D. G. and D. G. Thompson. A generalization of sampling without replacement from a finite universe, JASA, 1952, 47, pp. 663-685.

[5] Folsom, Ralph E., Jr. National Assessment Approach to Sampling Error Estimation, Sampling Error Monograph, Project No. 25U-796-5, Research Triangle Park, N.C.: Research Triangle Institute, 1974.

[6] Holt, Mary M. SURREGR: Standard errors of regression coefficients from sample survey data, Research Triangle Park, N.C.: Research Triangle Institute, May 1977.

The dummy variable procedure avoids assumptions of linearity of regression, and such dummy variables can also be used to encode terms for interactions between predictor variables. This procedure does, however, tend to proliferate model parameters, and for one of the predictor variables (specific project association), a criterion scaling approach was used. This approach (which is detailed in Subsection IV.A of this appendix) is quite straightforward when used alone; however, when used in conjunction with the dummy variable approach it yields appropriate tests only under fairly restrictive assumptions. No attempts were made to include polynomial term in the model to detect relationships other than linear ones.

For the current use of the regression model many of its assumptions were probably violated. In addition to the use of several noncontinuous predictor variables, one criterion variable was binary in nature (i.e., entering or not entering PSE) and as such violates the assumptions of classical regression analysis (homoscedastic and normal error variance). While there are approaches to model fitting that are more appropriate for categorical data, such as a log-linear model, they were not employed. The basic simplicity and ease of interpretation when using the original variables rather than transformations of the variables led to the choice, even though the assumptions of the model would be violated to some extent. In practice, the assumptions were not considered to be sufficiently violated to significantly distort the results of the analysis. For the binary criterion variable, the variance of the cell observations (which is a function of the cell proportions) did not vary greatly since most of the proportions were in the range of .25 to .75, and the maximum difference in variance of proportions in this range is less than .07 (i.e., (.50)(.50) - (.25)(.75) = .0625). Further, the parameter estimates are linear combinations of the dependent variable values; and since one is dealing with linear combinations of very large numbers of dependent variable values, one may appeal to the central limit theorem for assuming at least approximate normality.

Beyond the assumptions of the linear regression model, there are many additional limitations on analysis. In the main, these limitations are similar to those specified for the comparative descriptive analyses. Since such limitations have been discussed in detail elsewhere in this report (Section II, Appendix F), they will be considered only briefly here.

As detailed in Appendix D (Section VII) and in the base-year study report,[7] there were some problems of unreliability of variables used in the regression analysis. While the criterion varables used showed relatively high reliability in regards to temporal stability, they were less reliable over different modes of administration (self-administered or interview). Since about half of the data used in analysis were obtained as mail returns and half through telephone interview, this form of unreliability poses an obvious problem for analysis interpretation. Moreover, data obtained from secondary sources typically showed weak relationships to those obtained directly from the sample members.

There were also indications that the predictor variable measuring extent of UB participation (obtained retrospectively during the follow-up study) suffered from gross unreliability (although it is suspected that this resulted primarily from a basic weaknesses in the analogous base-year measure, to which the current measure was compared). The remaining variables within the predictor set (which were drawn from the base-year data base) could not be considered completely reliable, with the possible exception of specific UB project association. Some of the base-year variables had been subjected to a limited reliability investigation, and while the suggested reliability for these variables was quite high (less than 0.8 percent internal inconsistencies with other data elements) it was not perfect. Further, most of these base-year data elements contained some values that had been logically or stochastically imputed during the base-year study.

Another limitation that should be kept in mind in interpreting the results of the relational analysis is that there is a peculiar attenuation of the relationship between length of UB participation and high school completion. This extends to the measures of educational progress/persistence used in these analyses, highest grade completed and postsecondary entry, and, to the extent that educational expectations are partially tempered by reality, the attenuation also extends to that criterion variable. Specifically the attenuation results from the fact that the relationship between extent of UB participation and high school completion is directionally constrained. While discontinuation of program participation does not necessarily lead to discontinuation of educational progress in high school, dropping out of high school (discontinuation of high school progress) does lead to discontinuation of program participation. Thus, using length of program participation to "predict" educational outcomes may have lead to somewhat spurious results.

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[7] Burkheimer, G. J., Levinsohn, J. R., Koo, H. P., and French, A. M. Evaluation Study of the Upward Bound Program (Volume IV of A Study of the National Upward Bound and Talent Search Programs. Four Volumes). Research Triangle Park, N.C.: Center for Educational Research and Evaluation, Research Triangle Institute, April 1976. (ED 131-152)

One final caution to the reader involves the inability of any of the relational analyses to establish causation. While the existence of a relationship between two variables (or between one variable and a set of other variable) is a necessary condition for the existence of causation, it is not a sufficient one, even when one variable (or set of variables) logically precedes the other variable in time. There is always the possibility that some additional unconsidered (or unmeasured) variable, which is related to both the variables under consideration, is the causal agent. The establishment of causation is extremely difficult without the support of randomization that is available in the true experimental paradigm.

III. THE REGRESSION MODELS USED AND STEPS IN MODEL REDUCTION

The relational analyses performed were relatively straightforward. A general linear model was used to evaluate the associations between the three different outcome measures and the main effects and first-order interaction terms of the predictor variable set. The three regression models were mixed models since one of the predictor variables (age) was treated as a continuous variable, another (specific project associaton) was subjected to criterion scaling to produce a quasi-continuous variable,[8] and the remainder were treated as categorical variables and were represented by dummy variables in the reparameterized design matrix.

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[8] Because the use of the dummy variable approach to the categorical variate of specific project association would have vastly proliferated the number of model parameters (with associated computational problems and limitation of the perspective from which the analysis could be viewed), that variate was scaled to a quasi-continuous variable by the technique of criterion scaling (see Subsection IV.A of this appendix) prior to inclusion in the model. The unweighted project mean rather than the weighted project mean was used in this scaling, and the grand mean of the criterion-scaled variable was subtracted prior to creation of interaction terms.

The predictor variable set may be partitioned in this regression model into process (UB program related) variables and control (student background and contextual) variables. The regression model for predicting some outcome measure for some individual j, y_j, can be reparameterized as

$$y_j = \alpha_o + \sum_{\ell=1}^{r} \beta_\ell U_{\ell j} + \sum_{i=1}^{s} \tau_i V_{ij} + \sum_{\ell=1}^{r} \sum_{i=1}^{s} \gamma_{\ell i} U_{\ell j} V_{ij} + \sum_{m=1}^{t} \delta_m X_{mj} + \varepsilon_j$$

where

α_o, β_ℓ, τ_i, $\gamma_{\ell i}$, and δ_m are parameters of the regression equation;

$U_{\ell j}$ is the set of r process variates represeting extent of UB participation and specific UB project association for individual j;

V_{ij} is the set of s control variates for individual j;

$U_{\ell j}V_{ij}$ represents the r times s first order interactions of process and control variates for individual j;

X_{mj} represents the t first order interactions within the control variate set for individual j; and

ε_j is the residual error for individual j.

There are no interactions within the process variable set for this model, since the only variable in that set is extent of UB participation.

The specific dummy variables created for the model are given in Table G.1. While there were other reparameterization options, the particular dummy variables used reflected a specific theoretical framework to be used within a reduced main effects model, should such a model prove to be viable.[9] The set of predictor variables entering into the

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[9] For that framework, age was also recoded by simple subtraction so that the age of 16 took on a value of 0. The criterion-scaled variable of project membership was also recoded by subtraction of the mean (weighted or unweighted appropriately) value of that variable. These simple linear transformations do not affect the predictability of a model; however, they do influence the estimates of the regression parameters, generally yielding more interpretable results. Moreover, the recoding eliminates spuriously high correlations between dummy variables and interactions of these more-or-less continuous variables with those dummy variables, which can produce problems of matrix indeterminacy.

Table G.1

SPECIFIC DUMMY VARIABLES USED IN THE RELATIONAL ANALYSES

Variable	Value of Variable	Value Assigned to Associated Dummy Variables		
		1	2	3
Extent of UB Participation	0 Years	0	0	0
	1 Year	1	0	0
	2 Years	0	1	0
	3 Years	0	0	1
Race	Black	1	0	-
	White	0	0	-
	Other	0	1	-
Grade in 1973	10	1	0	-
	11	0	0	-
	12	0	1	-
Sex	Male	0	-	-
	Female	1	-	-
Poverty Status	Poverty	1	-	-
	Not Poverty	0	-	-
Academic Risk Status	Risk	1	-	-
	Not Risk	0	-	-

full model are given in Table G.2. Weighted and unweighted zero-order correlations among the main effect predictor variables and the three criterion variables are given in Tables G.3.1 through G.5.2 in Section V of this appendix. It should be noted from Table G.2 that the total number of variables in the model (73) is not great but that the degrees of freedom (697) are considerably greater. This is due to the fact that the criterion-scaled variable represents one model parameter but still carries the 53 degrees of freedom that would be associated with the categorical variable from which it was constructed; these additional degrees of freedom also carry over into interactions involving that variable.

The inclusion of specific project association as a control variate rather than as a process variate warrants some consideration. Project association applies both to former UB participants and to nonparticipants; however, the specific meaning of that variable is somewhat different for the two groups of individuals. For both participants and nonparticipants, specific project association represents a classification of individuals by site (i.e., a particular geographic area and a particular cluster of schools within that area). For UB participants, however, specific project association also represents a classification of individuals by a particular UB project, with all the unique process factors that are associated with that project. This initial ambiguity in the meaning of the specific project association factor is much less of a problem when one considers the full regression model, which includes the interaction terms involving extent of UB participation and specific project association. Those interaction terms capture any differential process factors associated with specific project association, since jointly they represent the differences between participants and nonparticipants within project. As long as these interaction terms are in the model, the process portion of specific project membership is partialled from the specific project association variable. On the other hand, the removal of these interaction terms from the model would imply that the process portion of specific project membership was not predictive, suggesting that only the contextual aspect of the variable possessed potential unique prediction capability. In either case, the specific project association variable reflects only the contextual classification.

Table G.2

PREDICTOR VARIABLES USED IN THE FULL REGRESSION MODEL

Variate	Variate Type	Variable Levels	Number of Dummy Variables	Degrees of Freedom
UB Participation	Process	4	3	3
Total Process (P)	--	--	3	3
UB Project Association	Process	a/	1	53
Age	Control	b/	1	1
Race	Control	3	2	2
Sex	Control	2	1	1
Grade Level 1973	Control	3	2	2
Poverty Status	Control	2	1	1
Academic Risk Status	Control	2	1	1
Total Control (C)	--	--	9	61
PxC Interactions	--	--	27	183
CxC Interactions	--	--	34	450

a/ A criterion scaling approach was used for this variable, creating a single variable that carried the degrees of freedom, appropriate to the categorical variable from which it was scaled.

b/ Treated as a continuous variable.

Since specific project association was a clustering factor in the initial sampling plan, inclusion of this variable in the model precluded appropriate estimation of the error terms required for statements of statistical confidence regarding national parameter estimates.[10] It was therefore necessary to use less appropriate approaches for estimating standard errors of regression parameters as long as specific project association remained in the model either as a "main effect" or in interaction with other variables. Moreover, in testing the full model, which contained 73 regression parameters and 697 degrees of freedom, the more convenient ordinary least squares approach, rather than a weighted least squares approach, was used. This restricted the resultant findings to description of functional relationships existing in the sample. Subsequent reduced models involving specific project association were solved by weighted least squares, but, as specified above, appropriate standard errors could not be computed, which again limited the generalizability of the results. Since project membership was considered an important variable, its inclusion in the model was considered desirable and the sacrifice in generalizability was accepted.

Alternate plans were developed for appropriate tests of the model should the project membership variables be removed in the step-down model reduction while other interaction terms remained. Since this eventuality did not obtain, these plans are not presented here.

Although it was possible to test each of the parameters of the regression model (both main-effect and interaction terms) individually for nullity and subsequently to remove them from the model should nullity be indicated, this was not an appropriate approach for the current situation. In the case of categorical predictor variables (including interaction terms), the nullity of a single contrast (dummy variable) does not imply the nullity of a factor.[11] Moreover, the purpose of these analyses did not call for such an approach, and a specific *a priori* rational approach to model reduction was employed. At any step in model reduction, prespecified sets of variables were removed from the model that was extant prior to that step and the resultant reduced model (with that set of variables excluded) was compared to the previous model (with that set of variables included).

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[10] The Taylorized deviations method available at RTI uses within-strata estimates of error, and clearly project membership will take on only two possible values in each stratum (only two projects were selected per stratum).

[11] For example, the race factor includes 2 dummy variables. The appropriate test for nullity of the race factor is therefore the simultaneous test of both dummy variables.

Conceptually, this requires a relatively simple test in which the proportion of variance explained by a model with k parameters, R_k^2, is tested against the proportion of variance accounted for after the removal of g of those parameters, R_{k-g}^2, for significant reduction in R^2 after removal of all g variables associated with those main effect or interaction terms that were to be eliminated in the particular model reduction step. Specifically, the expression

$$\frac{N-k-1}{g} \left[\frac{R_k^2 - R_{k-g}^2}{1 - R_k^2} \right] ,$$

where N is the total number of analysis units, is distributed as the random variable F with g and N-k-1 degrees of freedom, under the assumption of simultaneous nullity of the entire set of g removed variables and under other model-related assumptions.[12]

The first step in attempted model reduction for each of the three criterion variables was the simultaneous removal of *all* interaction terms. As stated previously, solutions for this step were obtained by ordinary least squares criteria. If, as a result of that removal, the associated reduction in R^2 was not significant by statistical test ($p<.05$) or magnitude test,[13] then the reduced model containing only main effects was to be used as a starting point for further reduction. If the reduction in R^2 as a result of removing all interaction terms was significant by both the statistical and magnitude test, then plans called for subsequent reduction of the model through a sequential approach of removing the various interaction and main effect factors according to a theoretical ordering for deletion that took into account the policy relevance of whatever terms remained in the model.[14] The criterion for removal of such terms was to be based on both magnitude and statistical tests as before. Since the reduction in R^2 by removing the entire set of interactive terms was not statistically significant for any of the three criterion variables, this alternative plan was not implemented. Tables G.6 through G.8 in Section V of this appendix provided summaries of these analyses. Tables G.9 through G.12 provide additional examinations of potential interactions through a more intuitive approach.

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[12] This is equivalent to a simultaneous test of nullity of the g variables removed. When removing single factors from the models at any given step, a more straightforward test of nullity of that factor was used.

[13] The magnitude test used was a 10 percent change in R^2. That is, if the value of $R_k^2 - R_{k-g}^2$ was equal to or less than $.1R_k^2$ then the reduction was not considered significant (e.g., for $R_k^2 = .50$, only reductions greater than .05 would be considered significant).

Having removed all interaction terms from the model, the next reduction attempted was the removal of the main effect of the contextual variable specific project association (since removal of that factor would lead to a model in which more applicable estimation procedures could be employed). As stated previously, solutions for this step were obtained using weighted least-squares criteria, but appropriate error terms could not be computed. The removal of the specific project association variable from the model did not bring about a statistically significant reduction of R^2 in any of the three regression models, and this factor was completely excluded from any additional model testing. See Tables G.13 through G.15 in Section V of this appendix for summaries of these analyses.

With the removal of project membership from the prediction equation, a relatively straightforward main effects model remained for each of the three criterion variables. Those models were further reduced, when possible, by removing the full set of generated contrasts (dummy variables) for each main effect factor.[15] Solutions for this and subsequent steps were obtained by using weighted least squares criteria and Taylorized deviations for standard error estimation. The order in which terms (or sets of terms) were removed from the model was determined by the respective relative increase in explained variation of the criterion (outcome) variable by those terms. The only additional factors removed from any of the three regression models were poverty status amd sex. See Tables G.16.1 through G.18 of Section V of this appendix for summaries of these analyses.

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[14] Since the number of possible empirical patterns of significant relationships was extremely large, no specific a priori plans for reduction were developed. Rather, such decisions were to be made dynamically by RTI and OPBE during actual analysis. Due to results of the first step in model reduction, it was not necessary to develop such alternate plans.

The parsimonious models remaining were then examined for their particular patterns of predictability for each criterion variable. Since the estimation techniques used in obtaining the regression parameters of the final main effects models were appropriate for the sample design that had been employed, the results could be considered national estimates for generalization beyond the sample; however, in interpreting the results, it should be kept in mind that the parameters of the prediction equation are only estimates, and that each estimate has an associated error variance. Summaries of these reduced models are provided in Tables G.19 through G.21 of Section V of this appendix.

IV. SPECIFIC ANALYSIS TECHNIQUES USED

Two of the techniques used in conducting the relational analyses are considered to be sufficiently unfamiliar to the general audience of this report to require some additional discussion. These techniques are criterion scaling and the Taylorized deviations approach to approximating error variances of regression parameters; both are detailed in this section.

A. Criterion Scaling

Faced with attempting to predict a criterion variable from categorical predictor variables, one is presented with a representation problem. Since categorical variables typically do not lend themselves to orthogonal decomposition, some other scaling or coding approach is required. The most common approach is that of using dummy variables; however, with the inclusion of a categorical variable with k levels, k-1 such dummy variables are required. This results in a proliferation of required model parameters which is further magnified when interaction terms are considered. Such an approach, while appropriate, can become intractable when applied within the constraints of available analysis software. Criterion scaling was advanced as an alternate approach to this problem. It is particularly important to realize that this approach to data representation does not produce any absolute scale factor. Rather, the scale is completely dependent on the particular criterion being used in the regression model.

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[15] Recall that a given main effect factor could generate more than one dummy variable.

The basic aim of criterion scaling is to produce a representation (scale value) for the categorical variable that maximizes the linear relationship between the categorical predictor variable and some criterion variable under consideration. Thus, given some criterion variable Y and a categorical predictor variable X with k categories, one desires a scale value S_i for each of the categories of X, X_i, $i \leftarrow 1, 2, \ldots, k$, so as to minimize the least squares criteria,

$$\sum_{i=1}^{k} \sum_{j=1}^{n_i} w_{ij} (Y_{ij} - S_i)^2 \quad ,$$

when these values are assigned to each of the n_i members in the respective categories X_i, where w_{ij} is a weight associated with individual j in category i, if considering weighted least squares, or 1, if considering unweighted least squares.

Obviously the only value of S_i minimizing these criteria are

$$S_i = \bar{Y}_i \ .$$

In other words, the scale value assigned to members of category X_i is the mean (weighted or unweighted appropriately) of the criterion measure within that category, thus the term criterion scaling.[16]

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[16] The dependence of this scaling technique to the particular criterion variable involved is clearly demonstrated in this presentation.

The linear relationship, r, between Y and S can be seen to be the correlation ratio (η) between Y and X. Since η^2 is the ratio of between category to total sums of squares,

$$\eta^2 = \frac{\sum_{i=1}^{k} n_i (\bar{Y}_j - \bar{Y})^2}{\sum_{i=1}^{k} \sum_{j=1}^{n_i} (Y_{ij} - \bar{Y})^2} ,$$

and since for the scaled variable, $r^2 = \eta^2$, an appropriate test exists for the nullity of r.[17]

It can be seen that the total relationship of the categories of X to Y, as reflected in η, have been transformed into a linear relationship between S and Y. As long as the appropriate degrees of freedom associated with the (X,Y) relationship (η) are maintained in the (S,Y) relationship (r), the scaling technique violates no assumptions of the general linear model.

Although some problems exist when using such a criterion-scaled variable in a multiple regression problem with other predictor variables, it is a useful and necessary technique for allowing an evaluation of the full regression models considered in this study.

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[17] The test available is identical to the F test for a one-way analysis of variance using the categories of X as levels of the independent variable and Y as the dependent variable.

B. Taylor Series Linearization for Regression Coefficients

1. Population Definitions

Consider a finite universe U with N units U(i). Associate with the i-th unit a variate value Y(i) and a row vector of (p+1) regressors

$$X(i) = \langle X_o(i)\ X_1(i)\ \dots\ X_p(i)\ \rangle . \tag{G.1}$$

The linear prediction equation for Y(i) of the form

$$y(i) = X\ (i)\ \beta , \tag{G.2}$$

which minimizes the sum of squared deviations

$$Q^2 = \sum_{i\varepsilon U} [Y(i) - X(i)\beta]^2 , \tag{G.3}$$

is the familiar least-squares regression equation where β is a solution to the so-called 'normal equations'

$$(X^T X)\beta = X^T Y , \tag{G.4}$$

where

$$X^T X \equiv \sum_{i\varepsilon U} X^T(i) X(i) ,$$

and

$$X^T Y \equiv \sum_{i\varepsilon U} X^T(i)\ Y(i).$$

If $X^T X$ has rank p+1, there is a unique solution for β in G.4; namely

$$\beta = (X^T X)^{-1} X^T Y . \tag{G.5}$$

If the p+1 equations represented by the matrix equation G.4 are not linearly independent, redundant equations can be replaced by independent linear restrictions on the βs. While the following development is limited to the full rank case, it is not difficult to extend the results directly to a particular restricted solution.

2. Estimation

Suppose that a sample S of n units is selected from the universe U. Let π(i) denote the probability that unit U(i) will be included in such a sample. Unbiased Horvitz-Thompson estimators for $X^T X$ and $X^T Y$ are

$$(x^T x) = \sum_{k\varepsilon s} X^T(k)\ X(k)\ /\pi(k) , \tag{G.6a}$$

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and

$$(x^T y) = \sum_{k \varepsilon s} X^T(k)\ Y(k)\ /\pi(k). \qquad (G.6b)$$

Using these estimators, we solve for b in the estimated set of normal equations

$$(x^T x)b = (x^T y). \qquad (G.7)$$

As an estimator of β the population vector of regression coefficients $b = (x^T x)^{-1}\ (x^T y)$ can be viewed as a matrix version of the combined ratio estimator $\hat{R} = (\hat{X})^{-1}(\hat{Y})$. This analogy will be strengthened by the form of the Taylor series approximation for $(b-\beta)$.

3. Taylor Series Variance Approximation

To generate the first order Taylor series approximation for b, we begin by evaluating the partial derivatives

$$\partial b/\partial\ (x^T y)_j\ , \qquad \text{for } j = 0, 1, \ldots.\ p\ , \qquad (G.8a)$$

and

$$\partial b/\partial\ (x^T x)_{jj'}\ , \qquad \text{for } j = 0, 1, \ldots.\ .p \qquad (G.8b)$$
$$j' = j,\ j+1, \ldots p\ ,$$

where $(x^T y)_j$ represents the j-th element in the (p+1) x 1 column vector $(x^T y)$ and $(x^T x)_{jj'}$ is the (jj')-th element of the (p+1) x (p+1) symmetric matrix $(x^T x)$.

First, we notice that

$$\{\partial(x^T x)b/\partial(x^T y)_j\} = (x^T x)\ \{\partial b/\partial\ (x^T y)_j\} \qquad (G.9)$$

$$= \partial(x^T y)/\partial(x^T y)_j\ .$$

Therefore,

$$(x^T x)\ \{\partial b/\partial(x^T y)_j\} = \delta_j\ , \qquad (G.10)$$

where

$$\delta_j(r) = \begin{cases} 1 \text{ if } & j = r \\ 0 \text{ otherwise} \end{cases}.$$

This allows us to write

$$\{\partial b/\partial(x^T y)_j\} = (x^T x)^{-1}\delta_j\ . \qquad (G.11)$$

Notice that δ_j is a (p+1) x 1 column vector with a 1 in row j and zeros elsewhere. To evaluate the partial derivative of b with respect to the elements of $(X^T X)$ we note that

$$\{\partial\ (x^T x) b / \partial\ (x^T x)_{jj'}\} = \partial\ (x^T y) / \partial\ (x^T x)_{jj'} = 0\ . \qquad (G.12)$$

Hence

$$(x^T x)\ \{\partial b / \partial (x^T x)_{jj'}\} + \left\{\partial (x^T x) / \partial (x^T x)_{jj'}\right\}\ b = 0 \qquad (G.13)$$

Recalling that $(X^T X)$ is symmetric, we see that

$$\{\partial (x^T x) / \partial (x^T x)_{jj'}\} = D_{jj'}\ , \qquad (G.14)$$

where the (r,c)-th element of $D_{jj'}$ is

$$d_{jj'}(rc) = [1 - \delta(jj')]\ \delta_j(r) \delta_{j'}(c) + \delta_{j'}(r) \delta_j(c)\ ,$$

with $\delta(jj') = 1$ if $j = j'$ and zero otherwise. This leads to

$$\{\partial b / \partial\ (x^T x)_{jj'}\} = -\ (x^T x)^{-1}\ D_{jj'} b\ , \qquad (G.15)$$

where $D_{jj'}$ is a (p+1) x (p+1) symmetric matrix having 1s in positions (jj') and (j'j) and zeros elsewhere.

Evaluating the partial derivatives in equations G.11 and G.15 at the point $<(x^T x) = X^T X;\ (x^T y) = X^T Y>$ we can approximate b with the first order Taylor series linearization

$$b \doteq \beta + \sum_{j=0}^{P}\ [(x^T y)_j - (X^T Y_j)] \{\partial b / (x^T y)_j\} \qquad (G.16)$$

$$+ \sum_{j=0}^{P}\ \sum_{j'=j}^{P}\ [(x^T x)_{jj'} - (X^T X)_{jj'}]\ \{\partial b / \partial (x^T x)_{jj'}\}\ ,$$

which becomes

$$b \doteq \beta + (X^T X)^{-1} \sum_{j=0}^{P}\ [(x^T y)_j - (X^T Y)_j]\ \delta_j$$

$$-\ (X^T X)^{-1} \sum_{j=0}^{P}\ \sum_{j'=j}^{P}\ [\ (x^T x)_{jj'} - (X^T X)_{jj'}] D_{jj'} \beta\ . \qquad (G.17)$$

Recalling the definitions of δ_j and $D_{jj'}$, it is easy to see that

$$\sum_{j=0}^{p} [(x^Ty)_j - (X^TY)_j] \delta_j = [(x^Ty) - (X^TY)] , \tag{G.18a}$$

and

$$\sum_{j=0}^{p} \sum_{j'=j}^{p} [(x^Tx)_{jj'} - (X^TX)_{jj'}] D_{jj'} = [(x^Tx) - (X^TX)]. \tag{G.18b}$$

This allows us to rewrite G.17 as

$$b \doteq \beta + (X^TX)^{-1} \{[(x^Ty) - (X^TY)] - [(x^Tx) - (X^TX)]\beta\}. \tag{G.19}$$

Finally, observing that $(X^TX)\beta = (X^TY)$ we have

$$b \doteq \beta + (X^TX)^{-1} [(x^Ty) - (x^Tx)\beta]. \tag{G.20}$$

To exploit the result in G.20 in order to approximate the sampling variance of b, we can define

$$z(k) = (X^TX)^{-1} [(x^Ty)_k - (x^Tx)_k\beta] , \tag{G.21}$$

where $(x^Ty)_k = X^T(k)\ Y(k)$,

and

$$(x^Tx)_k = X^T(k)\ X(k).$$

The corresponding Horvitz-Thompson estimator is

$$\begin{aligned} \hat{z} &= \sum_{k\varepsilon s} z(k)/\pi(k) \\ &= (X^TX)^{-1} [(x^Ty) - (x^Tx)\beta]. \end{aligned} \tag{G.22}$$

Combining G.20 and G.22 we see that

$$\hat{z} \doteq (b - \beta). \tag{G.23}$$

The result in G.22 leads one to the following approximation for the generalized mean-squared-error of b, our vector of p + 1 estimated regression coefficients:

$$\begin{aligned} \text{GMSE } \{b\} &= E_s \{(b-\beta)(b-\beta)^T\} \\ &\doteq E_s \{\hat{z}\ \hat{z}^T\} \\ &= \text{VAR } \{\hat{Z}\} , \end{aligned}$$

since $E_s \{\hat{Ƶ}\} = 0$, the (p+1) x 1 null vector.

If $VAR_D (\hat{T})$ is the sampling variance formula for an estimated total $\hat{T}$ from a particular sampling design (D), then the generalized mean-squared-error for a vector of (p+1) regression coefficients b estimated from D is approximately

$$GMSE \{b\} \doteq VAR_D \{\hat{Ƶ}\} , \qquad (G.25)$$

where $\hat{Ƶ}$ is the linearized statistic

$$\hat{Ƶ} = (X^T X)^{-1} \sum_{k \varepsilon s} [(x^T y)_k - (x^T x)_k \beta] / \pi(k) .$$

In the following section, we will exploit equation G.25 to produce the 'Taylor Series' variance estimator or more precisely, the generalized mean-squared-error estimator for b.

4. Taylor Series Variance Estimator

Recalling the Taylor series approximation for GMSE {b} developed in the previous section, it is clear that if $var_D(\hat{T})$ represents an appropriate variance estimator for the sample total $\hat{T}$ from design D, then $var_D(\hat{Ƶ})$ is the associated estimator for GMSE {b}. Since the linearized variate value $Ƶ(k)$ in G.21 is a function of the unknown population quantities $(X^T X)^{-1}$ and β, one is obligued to impose another level of approximation at this point. Instead of $Ƶ(k)$, we use

$$z(k) = (x^T x)^{-1} [(x^T y)_k - (x^T x)_k b] \qquad (G.26)$$

substituting our sample estimates for the unknown population parameters. It is interesting to note at this point that G.26 is a matrix analogue of the 'Taylorized' variate used to approximate the variance of the ratio $\hat{R} = (\hat{Y}/\hat{X})$. Expressing this ratio as $\hat{R} = (\hat{X})^{-1}(\hat{Y})$ and making the associations $(\hat{X})^{-1} <=> (x^T x)^{-1}$ and $(\hat{Y}) <=> (x^T y)$, the relationship between G.26 and the familiar

$$\begin{aligned} z_{\hat{R}}(k) &= [Y(k) - \hat{R}\, X\, (k)] / \hat{X} \\ &= (\hat{X})^{-1} [Y(k) - X(k)\, \hat{R}] \qquad (G.27) \end{aligned}$$

is obvious.

To illustrate the method, we can consider a stratified simple random cluster sample with h=1(1)H strata, and n(h) clusters selected from N(h) without replacement. If y(hik) is the variate value associated with the k-th unit [k=1(1) M(hi)] in cluster-i of stratum-h and X(hik) is the corresponding 1 x (p+1) row vector of regressors, we use the expanded cluster totals

$$(x^T y)_{hi} = N(h) \sum_{h=1}^{M(hi)} X^T(hik)\ Y(hik)/n(h)\ , \qquad (G.28a)$$

and

$$(x^T x)_{hi} = N(h) \sum_{k=1}^{M(hi)} X^T(hik)\ X\ (hik)/n(h)\ , \qquad (G.28b)$$

to form

$$\tilde{z}(hi) = [\ (x^T y)_{hi} - (x^T x)_{hi}\ b]\ , \qquad (G.29)$$

where

$$b = (x^T x)^{-1}\ (x^T y)\ ,$$

and

$$(x^T x) = \sum_{h=1}^{H} \sum_{i=1}^{n(h)} (x^T x)_{hi}\ ,$$

$$(x^T y) = \sum_{h=1}^{H} \sum_{i=1}^{n(h)} (x^T y)_{hi}\ .$$

Then, we calculate

$$\text{gmse}\ \{b\} = (x^T x)^{-1} \{ \sum_{h=1}^{H} \{1 - f(h)\}\ n(h)\ s^2_{\tilde{z}}\ (h)\}\ (x^T x)^{-1}, \qquad (G.30)$$

where f(h) = n(h)/N(h) and

$$s^2_{\tilde{z}(h)} = \sum_{i=1}^{n(h)} [\tilde{z}(hi) - \tilde{z}(h.)][\tilde{z}(hi) - \tilde{z}(h.)]^T/[n(h)-1]\ ,$$

with $\tilde{z}(h.) = \sum_{i=1}^{n(h)} \tilde{z}(hi)/n(h)$.

5. Statistical Inference

Researchers are often interested in testing hypotheses about relationships among population regression coefficients. While there is no rigorous solution to the general linear hypothesis problem in a finite population context, we suggest a heuristic approach which relies on the central limiting tendency of estimated coefficient vectors $\hat{b}$ to have approximately the multivariate normal sampling distribution with mean β and variance-covariance matrix $\mathrm{Var}_D\ \{\hat{z}\}$ as specified in G.25. To the extent that this approximation for the sampling distribution of b holds, one can justify the following approach for testing

$$H_o:\ C^T\beta = \delta \ \text{ versus } H_A:\ C^T\beta \neq \delta\ ; \qquad (G.31)$$

form the test statistic

$$T_c^2 = (C^Tb - \delta)^T[C^T\ \mathrm{var}_D(\hat{z})\ C]^{-1}(C^Tb - \delta)\ , \qquad (G.32)$$

and reject H_o if T_c^2 exceeds the upper α percentage point of the Chi-Square distribution with c = rank (C) degrees of freedom. This test is the multivariate analogue of the common large sample normal theory test.

When the degrees of freedom (df) associated with the estimated variance-covariance matrix $\mathrm{var}_D(z)$ drops below 60, A.32 may be viewed as the multivariate analogue of Student's t; namely, Hotelling's T^2 statistic. The F-transformed version of Hotelling's T^2 is expressed as

$$F = \{\frac{df + 1 - c}{(df)c}\} \quad T_c^2 \quad , \qquad (G.33)$$

where df is the degrees of freedom associated with $\mathrm{var}_D(z)$. For our example in the previous section we would recommend the approximation df ≐ n(+)-H where n(+) is the total number of clusters in the sample and H is the number of strata. The transformed statistic F is compared to the upper α percentage point of Fisher's F distribution with c and (df + 1 - c) degrees of freedom.

V. TABULAR SUMMARIES

Summaries of the various steps in model reduction, the final reduced regression models, and supporting results are presented in tabular form in this section. The tables, which constitute the remainder of this appendix are considered to be sufficiently annotated for interpretation without reference to textual material.

Table G.3.1

ZERO-ORDER CORRELATIONS AMONG PREDICTOR VARIABLES AND HIGHEST GRADE COMPLETED: UNWEIGHTED RESULTS

	Highest grade completed	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[a]	Black	Other ethnicity[b]	Sex[c]	Age	10th grade cohort	12th grade cohort	Poverty Status[d]
1 year UB participation	-06											
2 years UB participation	14	-24										
3 years UB participation	17	-22	-35									
Specific project association[a]	24	-01	17	03								
Black	14	01	16	15	28							
Other ethnicity[b]	-06	05	00	01	-08	-49						
Sex[c]	12	00	07	02	01	10	-04					
Age	13	07	09	-02	06	-02	05	-02				
10th grade cohort	-30	-04	-17	-05	-10	-05	-02	-06	-51			
12th grade cohort	35	02	08	04	10	04	-01	05	57	-42		
Poverty status[d]	00	07	14	10	07	22	03	10	07	-09	03	
Academic risk status[e]	-20	01	-03	-03	-08	07	02	-14	07	01	-03	04

NOTE: Correlation values given are for the "main effect" terms only, based on unweighted data of the 3,192 cases for whom no indeterminacy existed either for the predictor variables or for the highest grade completed. Decimal points implicitly preceeding each of the reported values have been omitted. With the exception of age and specific project association, all predictor variables are dummy variables reflecting group membership as indicated in the variable specification or in a footnote.

[a] Criterion-scaled to highest grade completed.

[b] Other than Black or white.

[c] The sex category "female" is the specific group coded.

[d] The category "within poverty status" is the specific group coded.

[e] The category "academic risk" is the specific group coded.

Table G.3.2

ZERO-ORDER CORRELATIONS AMONG PREDICTOR VARIABLES AND HIGHEST GRADE COMPLETED: WEIGHTED RESULTS

	Highest grade completed	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[a/]	Black	Other ethnicity[b/]	Sex[c/]	Age	10th grade cohort	12th grade cohort	Poverty Status[d/]
1 year UB participation	00											
2 years UB participation	04	-00										
3 years UB participation	04	-00	-01									
Specific project association[a/]	07	-01	01	-00								
Black	03	03	07	06	23							
Other ethnicity[b/]	-11	02	02	02	-03	-23						
Sex[c/]	15	00	02	01	08	14	-10					
Age	-02	02	04	02	-01	06	-05	-05				
10th grade cohort	-21	-02	-04	-03	-02	01	05	-07	-55			
12th grade cohort	32	02	03	03	08	00	-08	03	54	-51		
Poverty status[d/]	-08	04	06	05	10	38	16	08	03	02	-02	
Academic risk status[e/]	-26	00	00	-00	01	18	07	-14	22	-03	-02	17

NOTE: Correlation values given are for the "main effect" terms only, based on weighted data of the 3,192 cases for whom no indeterminacy existed either for the predictor variables or for the highest grade completed. Decimal points implicitly preceeding each of the reported values have been omitted. With the exception of age and specific project association, all predictor variables are dummy variables reflecting group membership as indicated in the variable specification or in a footnote.

[a/] Criterion-scaled to highest grade completed.

[b/] Other than Black or white.

[c/] The sex category "female" is the specific group coded.

[d/] The category "within poverty status" is the specific group coded.

[e/] The category "academic risk" is the specific group coded.

Table G.4.1

ZERO-ORDER CORRELATIONS AMONG PREDICTOR VARIABLES AND POSTSECONDARY ENTRY: UNWEIGHTED RESULTS

	Postsecondary entry	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[a]	Black	Other ethnicity[b]	Sex[c]	Age	10th grade cohort	12th grade cohort	Poverty Status[d]
1 year UB participation	-06											
2 years UB participation	10	-24										
3 years UB participation	19	-22	-35									
Specific project association[a]	23	-02	14	07								
Black	15	01	16	16	37							
Other ethnicity[b]	-07	05	00	01	-09	-49						
Sex[c]	12	01	07	02	03	10	-04					
Age	-00	06	09	-02	02	-02	05	-02				
10th grade cohort	-15	-04	-17	-05	-04	-05	-02	-06	-51			
12th grade cohort	14	01	08	04	06	04	-01	05	57	-42		
Poverty status[d]	-00	07	14	10	04	22	03	09	07	-09	03	
Academic risk status[e]	-18	01	-03	-02	-08	07	02	-14	07	01	-03	04

NOTE: Correlation values given are for the "main effect" terms only, based on unweighted data of the 3,203 cases for whom no indeterminacy existed either for the predictor variables or for postsecondary entry. Decimal points implicitly preceeding each of the reported values have been omitted. With the exception of age and specific project association, all predictor variables are dummy variables reflecting group membership as indicated in the variable specification or in a footnote.

[a] Criterion-scaled to postsecondary entry.

[b] Other than Black or white.

[c] The sex category "female" is the specific group coded.

[d] The category "within poverty status" is the specific group coded.

[e] The category "academic risk" is the specific group coded.

Table G.4.2

ZERO-ORDER CORRELATIONS AMONG PREDICTOR VARIABLES AND POSTSECONDARY ENTRY: WEIGHTED RESULTS

	Postsecondary entry	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[a]	Black	Other ethnicity[b]	Sex[c]	Age	10th grade cohort	12th grade cohort	Poverty Status[d]
1 year UB participation	00											
2 years UB participation	01	-00										
3 years UB participation	02	-00	-01									
Specific project association[a]	05	-01	01	00								
Black	00	03	07	06	26							
Other ethnicity[b]	-06	02	02	02	-05	-23						
Sex[c]	08	00	02	01	07	14	-10					
Age	-06	02	04	02	-03	06	-05	-05				
10th grade cohort	-02	-02	-04	03	-03	01	05	-07	-55			
12th grade cohort	03	02	03	03	04	00	-08	03	53	-51		
Poverty status[d]	-04	04	06	05	10	38	16	08	03	02	-02	
Academic risk status[e]	-12	00	00	-00	00	18	07	-14	22	-03	-02	17

NOTE: Correlation values given are for the "main effect" terms only, based on weighted data of the 3,203 cases for whom no indeterminacy existed either for the predictor variables or for postsecondary entry. Decimal points implicitly preceeding each of the reported values have been omitted. With the exception of age and specific project association, all predictor variables are dummy variables reflecting group membership as indicated in the variable specification or in a footnote.

[a] Criterion-scaled to postsecondary entry.

[b] Other than Black or white.

[c] The sex category "female" is the specific group coded.

[d] The category "within poverty status" is the specific group coded.

[e] The category "academic risk" is the specific group coded.

Table G.5.1

ZERO-ORDER CORRELATIONS AMONG PREDICTOR VARIABLES AND EDUCATIONAL EXPECTATION: UNWEIGHTED RESULTS

	Educational Expectations	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[a/]	Black	Other Ethnicity[b/]	Sex[c/]	Age	10th grade cohort	12th grade cohort	Poverty Status[d/]
1 year UB participation	-04											
2 years UB participation	13	-23										
3 years UB participation	20	-21	-35									
Specific project association[a/]	30	-03	10	11								
Black	22	-01	16	16	43							
Other ethnicity[b/]	-07	06	00	00	-07	-48						
Sex[c/]	03	-00	07	02	00	11	-04					
Age	-04	07	09	-02	-01	-01	05	-01				
10th grade cohort	-09	-05	-17	-05	-00	-04	-03	-07	-51			
12th grade cohort	12	03	08	04	05	04	-01	06	57	-42		
Poverty status[d/]	-02	07	15	11	01	22	03	10	07	-09	03	
Academic risk status[e/]	-17	00	-04	-01	-05	07	02	-13	06	01	-03	05

NOTE: Correlation values given are for the "main effect" terms only, based on unweighted data of the 2,994 cases for whom no indeterminacy existed either for the predictor variables or for educational expectations. Decimal points implicitly preceeding each of the reported values have been omitted. With the exception of age and specific project association, all predictor variables are dummy variables reflecting group membership as indicated in the variable specification or in a footnote.

[a/] Criterion-scaled to educational expectations.

[b/] Other than Black or white.

[c/] The sex category "female" is the specific group coded.

[d/] The category "within poverty status" is the specific group coded.

[e/] The category "academic risk" is the specific group coded.

Table G.5.2

ZERO-ORDER CORRELATIONS AMONG PREDICTOR VARIABLES AND EDUCATIONAL EXPECTATIONS: WEIGHTED RESULTS

	Educational Expectations	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[a]	Black	Other ethnicity[b]	Sex[c]	Age	10th grade cohort	12th grade cohort	Poverty Status[d]
1 year UB participation	01											
2 years UB participation	04	-00										
3 years UB participation	05	-00	-01									
Specific project association[a]	15	-02	00	01								
Black	08	03	07	07	33							
Other ethnicity[b]	-13	03	02	02	-03	-23						
Sex[c]	10	01	02	01	08	15	-09					
Age	-16	02	03	02	-06	07	-06	-04				
10th grade cohort	-05	-02	-04	-03	06	-02	05	-10	-57			
12th grade cohort	09	02	03	02	04	01	-08	04	54	-51		
Poverty status[d]	-12	04	06	06	16	37	17	07	02	02	-03	
Academic risk status[e]	-26	00	01	00	-01	17	08	-14	22	-05	-01	16

NOTE: Correlation values given are for the "main effect" terms only, based on weighted data of the 2,994 cases for whom no indeterminacy existed either for the predictor variables or for educational expectations. Decimal points implicitly preceeding each of the reported values have been omitted. With the exception of age and specific project association, all predictor variables are dummy variables reflecting group membership as indicated in the variable specification or in a footnote.

[a] Criterion-scaled to educational expectations.

[b] Other than Black or white.

[c] The sex category "female" is the specific group coded.

[d] The category "within poverty status" is the specific group coded.

[e] The category "academic risk" is the specific group coded.

Table G.6

ANALYSIS SUMMARY FOR TEST OF SIGNIFICANCE OF PREDICTION BY INTERACTION TERMS: HIGHEST GRADE COMPLETED

	Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F	R^2
Test of Full Model (including interactions)	Regression	k = 697	1044.0889	1.4980	1.61	.31016
	Error	N-k-1 = 2494	2322.2595	.9311		
	Total	N-1 = 3191	3366.3484			
	Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F	R^2
Test of "Main Effect" Model	Regression	m = 64	937.4556	14.6477	18.86	.27848
	Error	N-m-1 = 3127	2428.8928	.7767		
	Total	N-1 = 3191	3366.3484			
Test of Differential Prediction of the Two Models	$\left[\frac{R_k^2 - R_m^2}{1 - R_k^2}\right] \frac{N-k-1}{k-m} = .181 \sim F_{(k-m,N-k-1)}$					

NOTE: All tests reported are based on unweighted analysis, using ordinary least squares criteria, of the 3,192 cases for whom no indeterminacy existed for predictor variables or for highest grade completed.

Table G.7

ANALYSIS SUMMARY FOR TEST OF SIGNIFICANCE OF PREDICTION BY INTERACTION TERMS: POSTSECONDARY ENTRY

	Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F	R^2
Test of Full Model (including interactions)	Regression	k = 697	148.6674	.2133	.93	.20567
	Error	N-k-1 = 2505	574.1868	.2292		
	Total	N-1 = 3202	722.8542			
	Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F	R^2
Test of "Main Effect" Model	Regression	m = 64	121.8556	1.9040	9.94	.16858
	Error	N-m-1 = 3138	600.9986	.1915		
	Total	N-1 = 3202	722.8542			
Test of Differential Prediction of the Two Models	$\left[\frac{R_k^2 - R_m^2}{1 - R_k^2}\right] \frac{N-k-1}{k-m} = .180 \sim F_{(k-m,N-k-1)}$					

NOTE: All tests reported are based on unweighted analysis, using ordinary least squares criteria, of the 3,203 cases for whom no indeterminacy existed for predictor variables or for postsecondary entry.

Table G.8

ANALYSIS SUMMARY FOR TEST OF SIGNIFICANCE OF PREDICTION BY INTERACTION TERMS: EDUCATIONAL EXPECTATIONS

	Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F	R^2
Test of Full Model (including interactions)	Regression	k = 697	1292.2595	1.8540	1.03	.23866
	Error	N-k-1 = 2296	4122.4566	1.7955		
	Total	N-1 = 2993	5414.7161			
	Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F	R^2
Test of "Main Effect" Model	Regression	m = 64	1116.4045	17.4438	11.89	.20618
	Error	N-m-1 = 2929	4298.3116	1.4675		
	Total	N-1 = 2993	5414.7161			
Test of Differential Prediction of the Two Models	$\left[\frac{R_k^2 - R_m^2}{1 - R_k^2}\right] \frac{N-k-1}{k-m} = .155 \sim F_{(k-m,N-k-1)}$					

NOTE: All tests reported are based on unweighted analysis, using ordinary least squares criteria, of the 2,994 cases for whom no indeterminacy existed for predictor variables or for educational expectations.

Table G.9

PARAMETERS OF REGRESSION EQUATIONS FOR PREDICTING HIGHEST GRADE COMPLETED WITHIN SPECIFIC CLASSIFICATION VARIABLE SUBGROUPS

Classification Variable	Subgroup	Number of Cases	R^2	Weights and Standard Errors of Weights for Predictor Variables[a]												
				Intercept[b]	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[c]	Black	Other[d]	Sex[e]	Age[f]	10th grade cohort	12th grade cohort	Poverty status[g]	Academic risk status[h]
Extent of UB Participation	0 years	1,120	0.24	12.85 (.07)	--	--	--	0.26 (.10)	-0.00 (.06)	-0.23 (.08)	0.16 (.05)	-0.20 (.03)	-0.51 (.07)	0.58 (.07)	-0.25 (.06)	-0.35 (.05)
	1 year	416	0.26	12.60 (.16)	--	--	--	0.86 (.21)	0.19 (.13)	0.29 (.15)	0.04 (.10)	-0.20 (.06)	-0.63 (.16)	0.90 (.12)	-0.06 (.11)	-0.37 (.10)
	2 years	899	0.25	12.81 (.11)	--	--	--	1.04 (.13)	0.21 (.09)	0.11 (.10)	0.04 (.06)	-0.12 (.04)	-0.54 (.11)	0.66 (.07)	-0.06 (.07)	-0.31 (.06)
	3 years	757	0.24	12.88 (.10)	--	--	--	0.63 (.13)	0.21 (.08)	0.04 (.10)	0.18 (.06)	-0.02 (.04)	-0.60 (.10)	0.42 (.07)	-0.02 (.06)	-0.16 (.06)
Race	Black	1,706	0.28	12.60 (.07)	0.13 (.07)	0.43 (.06)	0.56 (.06)	0.62 (.10)	--	--	0.13 (.04)	-0.12 (.02)	-0.58 (.07)	0.63 (.05)	-0.07 (.05)	-0.24 (.04)
	White	936	0.24	12.90 (.07)	-0.14 (.10)	0.14 (.09)	0.31 (.09)	0.54 (.12)	--	--	0.11 (.06)	-0.19 (.04)	-0.51 (.09)	0.64 (.07)	-0.22 (.06)	-0.43 (.06)
	Other	550	0.29	12.52 (.11)	0.39 (.12)	0.48 (.10)	0.58 (.10)	0.71 (.13)	--	--	0.07 (.08)	-0.09 (.04)	-0.47 (.11)	0.54 (.09)	-0.11 (.08)	-0.35 (.08)
Sex	Male	1,485	0.25	12.73 (.06)	0.13 (.08)	0.38 (.06)	0.47 (.07)	0.58 (.10)	0.06 (.06)	-0.06 (.07)	--	-0.14 (.03)	-0.51 (.07)	0.55 (.06)	-0.13 (.05)	-0.34 (.05)
	Female	1,707	0.28	12.84 (.06)	0.02 (.07)	0.28 (.06)	0.44 (.06)	0.66 (.09)	0.08 (.06)	-0.06 (.07)	--	-0.13 (.03)	-0.56 (.07)	0.68 (.05)	-0.17 (.05)	-0.30 (.04)
Poverty Status	Poverty Level	2,065	0.29	12.41 (.07)	0.16 (.06)	0.42 (.05)	0.57 (.06)	0.68 (.08)	0.15 (.05)	0.03 (.06)	0.11 (.04)	-0.12 (.02)	-0.54 (.06)	0.61 (.05)	--	-0.28 (.04)
	Not Poverty Level	1,127	0.27	12.81 (.07)	-0.02 (.09)	0.23 (.08)	0.31 (.08)	0.56 (.11)	0.01 (.07)	-0.11 (.08)	0.15 (.05)	-0.15 (.03)	-0.51 (.08)	0.64 (.07)	--	-0.36 (.05)
Academic Risk Status	Risk	1,396	0.25	12.31 (.07)	0.08 (.08)	0.35 (.07)	0.40 (.07)	0.63 (.10)	0.14 (.07)	-0.02 (.08)	0.13 (.05)	-0.14 (.03)	-0.52 (.07)	0.37 (.06)	-0.12 (.05)	--
	Not risk	1,796	0.25	12.77 (.06)	0.06 (.07)	0.30 (.06)	0.35 (.06)	0.63 (.09)	0.02 (.05)	-0.16 (.06)	0.11 (.04)	-0.13 (.03)	-0.52 (.06)	0.67 (.05)	-0.17 (.04)	
Grade Cohort	10th grade	613	0.16	12.49 (.07)	-0.12 (.10)	0.24 (.10)	0.27 (.09)	0.38 (.13)	0.03 (.08)	-0.15 (.09)	0.05 (.06)	-0.17 (.03)	--	--	-0.16 (.07)	-0.30 (.06)
	11th grade	1,213	0.16	12.68 (.06)	-0.05 (.08)	0.28 (.06)	0.52 (.07)	0.51 (.10)	0.10 (.06)	-0.03 (.07)	0.07 (.05)	-0.11 (.03)	--	--	-0.12 (.05)	-0.29 (.05)
	12th grade	1,366	0.17	13.10 (.07)	0.30 (.09)	0.43 (.07)	0.52 (.08)	0.81 (.12)	0.08 (.07)	-0.05 (.09)	0.19 (.06)	-0.15 (.03)	--	--	-0.16 (.06)	-0.34 (.06)

NOTE: The regression analyses reported in this table were computed using unweighted data and ordinary least squares criteria. With the exception of age and project membership, all predictor variables are dummy variables reflecting group membership as indicated in the column label or in a footnote. When examining within group regression for a particular classification variable, the dummy variable(s) corresponding to that classification could not be used in prediction.

[a] Standard errors associated with a given regression weight are presented, within parentheses, immediately below the associated weight.

[b] Except as modified by exclusion of some predictor variables or as noted elsewhere, the dummy variables and continuous variables were coded such that this value represents the predicted value of highest grade completed for a white, non-risk, male nonparticipant who was not poverty level, was 16 years old, and was in the eleventh grade cohort associated with an "average" project.

[c] This variable is criterion scaled to highest grade completed and was recoded by subtracting its mean value. Due to the nature of criterion scaling, the reported standard error is an underestimate.

[d] Other than Black or white.

[e] The sex category "female" is the specific group coded.

[f] Age has generally been recoded by subtraction of 16; however, to remove the relationship between grade cohort and age, age was recoded by subtracting grade + 5 when grade was the classification variable considered.

[g] The category "within poverty status" is the specific group coded.

[h] The category "academic risk" is the specific group coded.

Table G.10

PARAMETERS OF REGRESSION EQUATIONS FOR PREDICTING POSTSECONDARY ENTRY WITHIN SPECIFIC CLASSIFICATION VARIABLE SUBGROUPS

Classification Variable	Subgroup	Number of Cases	R^2	Weights and Standard Errors of Weights for Predictor Variables[a]												
				Intercept[b]	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[c]	Black	Other[d]	Sex[e]	Age[f]	10th grade cohort	12th grade cohort	Poverty status[g]	Academic risk status[h]
Extent of UB Participation	0 years	1,123	0.14	0.70 (.03)	--	--	--	0.61 (.13)	-0.02 (.03)	-0.15 (.04)	0.12 (.03)	-0.07 (.02)	-0.14 (.04)	0.08 (.04)	-0.11 (.03)	-0.17 (.03)
	1 year	417	0.15	0.53 (.07)	--	--	--	0.74 (.23)	0.07 (.06)	0.13 (.07)	0.07 (.03)	-0.07 (.03)	-0.12 (.07)	0.10 (.06)	-0.04 (.05)	-0.13 (.05)
	2 years	901	0.15	0.67 (.05)	--	--	--	0.87 (.15)	0.13 (.04)	0.03 (.05)	0.02 (.03)	-0.06 (.02)	-0.26 (.05)	0.13 (.03)	0.00 (.03)	-0.13 (.03)
	3 years	762	0.07	0.79 (.05)	--	--	--	0.62 (.14)	0.09 (.04)	0.05 (.05)	0.02 (.03)	-0.02 (.02)	-0.13 (.04)	0.02 (.03)	-0.02 (.03)	-0.06 (.03)
Race	Black	1,713	0.16	0.59 (.04)	0.06 (.03)	0.21 (.03)	0.28 (.03)	0.72 (.12)	--	--	0.06 (.02)	-0.06 (.01)	-0.17 (.03)	0.12 (.02)	-0.04 (.02)	-0.07 (.02)
	White	938	0.14	0.70 (.04)	-0.07 (.05)	0.03 (.05)	0.17 (.05)	0.65 (.14)	--	--	0.08 (.03)	-0.05 (.02)	-0.11 (.05)	0.11 (.04)	-0.09 (.03)	-0.23 (.03)
	Other	552	0.21	0.48 (.06)	0.23 (.06)	0.23 (.05)	0.37 (.05)	0.63 (.16)	--	--	0.04 (.04)	-0.04 (.02)	-0.17 (.06)	0.12 (.03)	-0.02 (.04)	-0.15 (.04)
Sex	Male	1,490	0.19	0.64 (.03)	0.07 (.04)	0.20 (.03)	0.31 (.03)	0.83 (.12)	0.01 (.03)	-0.04 (.03)	--	-0.05 (.01)	-0.15 (.03)	0.09 (.03)	-0.07 (.03)	-0.16 (.02)
	Female	1,713	0.13	0.71 (.03)	0.01 (.04)	0.11 (.03)	0.20 (.03)	0.60 (.10)	0.03 (.03)	-0.04 (.03)	--	-0.06 (.01)	-0.14 (.03)	0.14 (.03)	-0.06 (.02)	-0.11 (.02)
Poverty Status	Poverty Level	2,070	0.19	0.49 (.03)	0.08 (.03)	0.20 (.03)	0.30 (.03)	0.71 (.10)	0.06 (.03)	0.01 (.03)	0.06 (.02)	-0.05 (.01)	-0.17 (.03)	0.11 (.02)	--	-0.10 (.02)
	Not Poverty Level	1,133	0.16	0.68 (.03)	0.02 (.05)	0.09 (.04)	0.19 (.04)	0.70 (.13)	0.01 (.03)	-0.08 (.04)	0.07 (.03)	-0.06 (.02)	-0.11 (.04)	0.11 (.03)	--	-0.16 (.03)
Academic Risk Status	Risk	1,401	0.20	0.42 (.04)	0.06 (.04)	0.16 (.03)	0.31 (.03)	0.86 (.12)	0.08 (.03)	-0.01 (.04)	0.08 (.03)	-0.04 (.02)	-0.17 (.04)	0.12 (.03)	-0.04 (.03)	--
	Not risk	1,802	0.10	0.69 (.03)	0.04 (.03)	0.15 (.03)	0.21 (.03)	0.57 (.10)	-0.01 (.03)	-0.05 (.03)	0.05 (.02)	-0.07 (.01)	-0.12 (.03)	0.12 (.03)	-0.08 (.03)	--
Grade Cohort	10th grade	613	0.18	0.66 (.04)	-0.00 (.07)	0.06 (.06)	0.23 (.05)	0.56 (.18)	0.01 (.05)	-0.12 (.06)	0.07 (.04)	-0.08 (.02)	--	--	-0.11 (.04)	-0.20 (.04)
	11th grade	1,213	0.16	0.64 (.04)	-0.07 (.04)	0.12 (.03)	0.26 (.04)	0.84 (.13)	0.04 (.03)	-0.03 (.04)	0.04 (.03)	-0.05 (.02)	--	--	-0.03 (.03)	-0.13 (.03)
	12th grade	1,377	0.14	0.62 (.03)	0.17 (.04)	0.21 (.03)	0.26 (.03)	0.66 (.12)	0.03 (.03)	-0.02 (.04)	0.09 (.02)	-0.04 (.01)	--	--	-0.06 (.02)	-0.10 (.02)

NOTE: The regression analyses reported in this table were computed using unweighted data and ordinary least squares criteria. With the exception of age and project membership, all predictor variables are dummy variables reflecting group membership as indicated in the column label or in a footnote. When examining within group regression for a particular classification variable, the dummy variable(s) corresponding to that classification could not be used in prediction.

[a] Standard errors associated with a given regression weight are presented, within parentheses, immediately below the associated weight.

[b] Except as modified by exclusion of some predictor variables or as noted elsewhere, the dummy variables and continuous variables were coded such that this value represents the predicted value of postsecondary entry for a white, non-risk, male nonparticipant who was not poverty level, was 16 years old, and was in the eleventh grade cohort associated with an "average" project.

[c] This variable is criterion scaled to postsecondary entry and was recoded by subtracting its mean value. Due to the nature of criterion scaling, the reported standard error is an underestimate.

[d] Other than Black or white.

[e] The sex category "female" is the specific group coded.

[f] Age has generally been recoded by subtraction of 16; however, to remove the relationship between grade cohort and age, age was recoded by subtracting grade + 5 when grade was the classification variable considered.

[g] The category "within poverty status" is the specific group coded.

[h] The category "academic risk" is the specific group coded.

Table G.11

PARAMETERS OF REGRESSION EQUATIONS FOR PREDICTING EDUCATIONAL EXPECTATION
WITHIN SPECIFIC CLASSIFICATION VARIABLE SUBGROUPS

Classification Variable	Subgroup	Number of Cases	R^2	Weights and Standard Errors of Weights for Predictor Variables[a]												
				Intercept[b]	1 year UB participation	2 years UB participation	3 years UB participation	Specific project association[c]	Black	Other[d]	Sex[e]	Age[f]	10th grade cohort	11th grade cohort	Poverty status[g]	Academic risk status[h]
Extent of UB Participation	0 years	1,074	0.18	3.52 (.10)	--	--	—	0.61 (.10)	0.24 (.10)	−0.31 (.12)	−0.05 (.08)	−0.29 (.05)	−0.29 (.10)	0.39 (.10)	−0.46 (.08)	−0.53 (.08)
	1 year	361	0.16	2.79 (.21)	—	—	--	0.46 (.18)	0.51 (.18)	0.31 (.19)	0.01 (.13)	−0.16 (.08)	0.17 (.22)	0.90 (.16)	−0.04 (.15)	−0.30 (.13)
	2 years	844	0.15	3.60 (.15)	—	—	—	0.92 (.12)	0.33 (.13)	0.21 (.15)	−0.15 (.09)	−0.13 (.05)	−0.26 (.16)	0.33 (.10)	−0.09 (.10)	−0.30 (.09)
	3 years	715	0.09	3.73 (.15)	—	--	—	0.56 (.12)	0.35 (.13)	0.16 (.15)	−0.03 (.08)	−0.04 (.05)	−0.21 (.13)	0.05 (.10)	−0.07 (.09)	−0.22 (.08)
Race	Black	1,587	0.16	3.30 (.10)	0.39 (.10)	0.66 (.08)	0.81 (.08)	0.72 (.09)	--	--	−0.03 (.06)	−0.15 (.03)	−0.22 (.09)	0.29 (.07)	−0.17 (.07)	−0.27 (.06)
	White	894	0.15	3.37 (.11)	0.11 (.15)	0.42 (.13)	0.70 (.14)	0.60 (.11)	—	—	−0.03 (.09)	−0.28 (.06)	−0.15 (.13)	0.54 (.11)	−0.41 (.10)	−0.48 (.09)
	Other	513	0.26	2.96 (.16)	0.70 (.16)	0.97 (.14)	1.14 (.15)	0.60 (.14)	--	--	−0.28 (.11)	−0.13 (.06)	−0.24 (.17)	0.43 (.13)	−0.11 (.11)	−0.50 (.11)
Sex	Male	1,376	0.22	3.20 (.09)	0.30 (.11)	0.70 (.09)	0.80 (.09)	0.66 (.09)	0.23 (.09)	0.06 (.10)	--	−0.17 (.04)	−0.13 (.10)	0.39 (.08)	−0.26 (.07)	−0.43 (.07)
	Female	1,618	0.19	3.21 (.09)	0.37 (.10)	0.56 (.08)	0.77 (.09)	0.71 (.08)	0.20 (.08)	−0.12 (.10)	—	−0.18 (.04)	−0.25 (.10)	0.36 (.08)	−0.26 (.07)	−0.33 (.06)
Poverty Status	Poverty Level	1,918	0.22	2.68 (.10)	0.53 (.09)	0.79 (.07)	0.97 (.08)	0.60 (.08)	0.37 (.08)	0.15 (.09)	−0.07 (.06)	−0.17 (.03)	−0.20 (.09)	0.37 (.07)	--	−0.30 (.06)
	Not Poverty Level	1,076	0.19	3.40 (.10)	0.09 (.14)	0.41 (.11)	0.54 (.11)	0.79 (.10)	0.13 (.10)	−0.14 (.12)	−0.05 (.08)	−0.16 (.05)	−0.15 (.11)	0.36 (.10)	--	−0.48 (.08)
Academic Risk Status	Risk	1,284	0.22	2.70 (.10)	0.48 (.12)	0.74 (.09)	0.98 (.10)	0.65 (.09)	0.29 (.10)	−0.06 (.11)	−0.03 (.07)	−0.15 (.04)	−0.27 (.10)	0.30 (.08)	−0.20 (.08)	--
	Not risk	1,710	0.17	3.32 (.08)	0.22 (.10)	0.52 (.08)	0.63 (.08)	0.71 (.08)	0.18 (.08)	0.02 (.09)	−0.09 (.06)	−0.19 (.04)	−0.13 (.09)	0.43 (.07)	−0.28 (.06)	--
Grade Cohort	10th grade	573	0.20	3.46 (.12)	0.43 (.20)	0.65 (.17)	0.70 (.15)	0.34 (.14)	0.23 (.14)	−0.26 (.17)	−0.15 (.11)	−0.26 (.06)	--	--	−0.31 (.11)	−0.56 (.11)
	11th grade	1,148	0.22	3.15 (.10)	−0.01 (.12)	0.56 (.10)	0.85 (.10)	0.75 (.10)	0.28 (.10)	0.02 (.11)	−0.02 (.07)	−0.16 (.04)	--	—	−0.23 (.08)	−0.29 (.07)
	12th grade	1,273	0.20	3.38 (.09)	0.64 (.11)	0.68 (.09)	0.76 (.10)	0.80 (.09)	0.14 (.10)	−0.01 (.11)	−0.06 (.07)	−0.15 (.04)	--	--	−0.24 (.07)	−0.37 (.07)

NOTE: The regression analyses reported in this table were computed using unweighted data and ordinary least squares criteria. With the exception of age and project membership, all predictor variables are dummy variables reflecting group membership as indicated in the column label or in a footnote. When examining within group regression for a particular classification variable, the dummy variable(s) corresponding to that classification could not be used in prediction.

[a] Standard errors associated with a given regression weight are presented, within parentheses, immediately below the associated weight.

[b] Except as modified by exclusion of some predictor variables or as noted elsewhere, the dummy variables and continuous variables were coded such that this value represents the predicted value of educational expectations for a white, non-risk, male nonparticipant who was not poverty level, was 16 years old, and was in the eleventh grade cohort associated with an "average" project.

[c] This variable is criterion scaled to educational expectation and was recoded by subtracting its mean value. Due to the nature of criterion scaling, the reported standard error is an underestimate.

[d] Other than Black or white.

[e] The sex category "female" is the specific group coded.

[f] Age has generally been recoded by subtraction of 16; however, to remove the relationship between grade cohort and age, age was recoded by subtracting grade + 5 when grade was the classification variable considered.

[g] The category "within poverty status" is the specific group coded.

[h] The category "academic risk" is the specific group coded.

Table G.12

COMPARISONS OF PARAMETERS OF WITHIN SUBGROUP REGRESSION EQUATIONS FOR THE THREE EDUCATIONAL OUTCOMES

Prediction Term With Which Parameter is Associated[a]	Number of Possible Pairwise Comparisons[b]	Number and Percentage of Pairwise Nonoverlapping 95% Confidence Intervals for Each Outcome Measure[c]		
		Highest Grade Completed	Postsecondary Entry	Educational Expectations
1 year UB participation	66	5 (7.5%)	6 (9.1%)	7 (10.6%)
2 years UB participation	66	0 (0.0%)	4 (6.1%)	2 (3.0%)
3 years UB participation	66	0 (0.0%)	0 (0.0%)	0 (0.0%)
Specific project association[d]	120	6 (5.0%)	0 (0.0%)	1 (0.8%)
Black	78	0 (0.0%)	1 (1.3%)	0 (0.0%)
Other[e]	78	1 (1.3%)	4 (5.1%)	0 (0.0%)
Sex[f]	91	0 (0.0%)	0 (0.0%)	0 (0.0%)
Age[g]	120	3 (2.5%)	0 (0.0%)	2 (1.6%)
Tenth Grade Cohort	78	0 (0.0%)	0 (0.0%)	0 (0.0%)
Twelfth Grade Cohort	78	1 (1.3%)	5 (6.4%)	10 (12.8%)
Poverty Status[h]	91	0 (0.0%)	0 (0.0%)	0 (0.0%)
Academic risk status[i]	91	1 (1.1%)	1 (1.1%)	0 (0.0%)

NOTE: The comparisons reported in this table are based on the regression solutions presented in Tables G.9 through G.11, which were computed for unweighted ordinary least squares solutions. The comparisons involved constructing 95% confidence intervals (two standard deviations) for each regression parameter, performing all pairwise comparisons of such confidence intervals within a particular prediction term for a particular outcome measure, and noting the number of cases in which the confidence intervals did not overlap.

[a] With the exception of age and project membership, all predictor terms are dummy variables reflecting group membership as indicated in the row heading or in a footnote.

[b] When examining within group regression for a particular classification variable, the dummy variable(s) corresponding to that classification could not be used in prediction; therefore, the number of equations in which a term appeared varied from term to term. Letting n equal the number of such regression equations in which a term appeared, the number of pairwise comparisons is given by $n(n-1)/2$.

[c] Percentages are given in parentheses.

[d] This variable is criterion scaled to the specific outcome measure and was recoded by subtracting its mean value. Due to the nature of criterion scaling, the standard errors used are estimates.

[e] Other than Black or white.

[f] The sex category "female" is the specific group coded.

[g] Age has generally been recoded by subtraction of 16; however, to remove the relationship between grade cohort and age, age was recoded by subtracting grade + 5 when grade was the classification variable considered.

[h] The category "within poverty status" is the specific group coded.

[i] The category "academic risk" is the specific group coded.

Table G.13

ANALYSIS SUMMARY FOR TEST OF SIGNIFICANCE OF PREDICTION BY THE SPECIFIC PROJECT ASSOCIATION FACTOR: HIGHEST GRADE COMPLETED

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error	F[a]	R^2[b]
Total Model	64	3127	14.87*	.233 (.233)
Age	1	3127	167.60*	--
Extent of UB Participation	3	3127	2.62*	--
Race	2	3127	13.83*	--
Grade	2	3127	276.97*	--
Poverty Status	1	3127	12.56*	--
Academic Risk	1	3127	125.99*	--
Sex	1	3127	22.55*	--
Specific Project Association[c]	53	3127	0.03	--

NOTE: The tests reported are based on weighted least squares analyses of the 3192 cases for whom no indeterminacy existed for predictor variables or for highest grade completed.

[a] An asterisk indicates statistical significance at the .05 level.

[b] Two values of R^2 are given. The uppermost value is for the complete "main effect" model; the value in parenthesis is for the model resulting after removing specific project membership from the prediction equation.

[c] Represents the variable criterion-scaled to highest grade completed.

Table G.14

ANALYSIS SUMMARY FOR TEST OF SIGNIFICANCE OF PREDICTION BY THE SPECIFIC PROJECT ASSOCIATION FACTOR: POSTSECONDARY ENTRY

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error	F[a]	R^2[b]
Total Model	64	3138	8.35*	.146 (.136)
Age	1	3138	70.40*	--
Extent of UB Participation	3	3138	3.57*	--
Race	2	3138	13.52*	--
Grade	2	3138	33.31*	--
Poverty Status	1	3138	13.05*	--
Academic Risk	1	3138	132.47*	
Sex	1	3138	44.82*	--
Specific Project Association[c]	53	3138	0.68	--

NOTE: The tests reported are based on weighted least squares analyses of the 3203 cases for whom no indeterminacy existed for predictor variables or for postsecondary entry.

[a] An asterisk indicates statistical significance at the .05 level.

[b] Two values of R^2 are given. The uppermost value is for the complete "main effect" model; the value in parenthesis is for the model resulting after removing specific project membership from the prediction equation.

[c] Represents the variable criterion-scaled to postsecondary entry.

Table G.15

ANALYSIS SUMMARY FOR TEST OF SIGNIFICANCE OF PREDICTION BY THE SPECIFIC PROJECT ASSOCIATION FACTOR: EDUCATIONAL EXPECTATIONS

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error	F[a]	R^2[b]
Total Model	64	2929	9.69*	.175 (.164)
Age	1	2929	158.26*	--
Extent of UB Participation	3	2929	5.07*	--
Race	2	2929	30.44*	--
Grade	2	2929	65.59*	--
Poverty Status	1	2929	54.88*	--
Academic Risk	1	2929	118.75*	--
Sex	1	2929	1.14	--
Specific Project Association[c]	53	2929	0.75	--

NOTE: The tests reported are based on weighted least squares analyses of the 2994 cases for whom no indeterminacy existed for predictor variables or for educational expectations.

[a] An asterisk indicates statistical significance at the .05 level.

[b] Two values of R^2 are given. The uppermost value is for the complete "main effect" model; the value in parenthesis is for the model resulting after removing specific project membership from the prediction equation.

[c] Represents the variable criterion-scaled to educational expectations.

Table G.16.1

ANALYSIS SUMMARY OF TESTS FOR REDUCTION OF MAIN EFFECTS MODEL: HIGHEST GRADE COMPLETED

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error[a]	F[b]	R^2[c]
Total Model	11	27	16.46*	.233 (.225)
Age	1	27	17.31*	—
Extent of UB Participation	3	27	9.73*	—
Race	2	27	7.65*	—
Grade	2	27	21.55*	—
Poverty Status	1	27	1.55	—
Academic Risk	1	27	16.08*	—
Sex	1	27	1.97	—

NOTE: The tests reported are based on weighted least squares analyses of the 3192 cases for whom no indeterminacy existed for predictor variables or for highest grade completed. Actual tests are based on a Taylorized deviation approach, as defined in Section IV of this Appendix.

[a] The Taylorized deviation approach determines the appropriate estimate of the error term within sampling strata; the associated degrees of freedom is thus the total number of primary sampling units (54) minus the number of strata (27).

[b] An asterisk indicates statistical significance at the .05 level.

[c] Values of R^2 were determined from the weighted least squares solution; two values are given. The uppermost value is for the model presented; the value in parenthesis is for the model resulting after removing all nonsignificant predictors from the equation.

Table G.16.2

ANALYSIS SUMMARY OF TESTS FOR REDUCTION OF MAIN EFFECTS MODEL: HIGHEST GRADE COMPLETED AFTER REMOVING SEX FROM THE MODEL

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error[a]	F[b]	R^2[c]
Total Model	10	27	16.78*	.227 (.225)
Age	1	27	19.91*	--
Extent of UB Participation	3	27	9.47*	--
Race	2	27	9.76*	--
Grade	2	27	22.48*	--
Poverty Status	1	27	1.26	--
Academic Risk	1	27	17.65*	--

NOTE: The tests reported are based on weighted least squares analyses of the 3192 cases for whom no indeterminacy existed for predictor variables or for highest grade completed. Actual tests are based on a Taylorized deviation approach, as defined in Section IV of this Appendix.

[a] The Taylorized deviation approach determines the appropriate estimate of the error term within sampling strata; the associated degrees of freedom is thus the total number of primary sampling units (54) minus the number of strata (27).

[b] An asterisk indicates statistical significance at the .05 level.

[c] Values of R^2 were determined from the weighted least squares solution; two values are given. The uppermost value is for the model presented; the value in parenthesis is for the model resulting after removing all nonsignificant predictors from the equation.

Table G.16.3

ANALYSIS SUMMARY OF TESTS FOR REDUCTION OF MAIN EFFECTS MODEL: HIGHEST GRADE COMPLETED AFTER REMOVING POVERTY STATUS FROM THE MODEL

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error[a]	F[b]	R^2[c]
Total Model	10	27	18.90*	.230 (.225)
Age	1	27	17.10*	--
Extent of UB Participation	3	27	10.43*	--
Race	2	27	6.90*	--
Grade	2	27	20.56*	--
Academic Risk	1	27	16.54*	--
Sex	1	27	1.73	--

NOTE: The tests reported are based on weighted least squares analyses of the 3192 cases for whom no indeterminacy existed for predictor variables or for highest grade completed. Actual tests are based on a Taylorized deviation approach, as defined in Section IV of this Appendix.

[a] The Taylorized deviation approach determines the appropriate estimate of the error term within sampling strata; the associated degrees of freedom is thus the total number of primary sampling units (54) minus the number of strata (27).

[b] An asterisk indicates statistical significance at the .05 level.

[c] Values of R^2 were determined from the weighted least squares solution; two values are given. The uppermost value is for the model presented; the value in parenthesis is for the model resulting after removing all nonsignificant predictors from the equation.

Table G.17

ANALYSIS SUMMARY OF TESTS FOR REDUCTION OF MAIN EFFECTS MODEL: POSTSECONDARY ENTRY

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error[a]	F[b]	R^2[c]
Total Model	11	27	13.56*	.136 (.132)
Age	1	27	16.58*	--
Extent of UB Participation	3	27	16.60*	--
Race	2	27	5.09*	--
Grade	2	27	4.71*	--
Poverty Status	2	27	28.65*	--
Academic Risk	1	27	1.42	--
Sex	1	27	5.75*	--

NOTE: The tests reported are based on weighted least squares analyses of the 3203 cases for whom no indeterminacy existed for predictor variables or for postsecondary entry. Actual tests are based on a Taylorized deviation approach, as defined in Section IV of this Appendix.

[a] The Taylorized deviation approach determines the appropriate estimate of the error term within sampling strata; the associated degrees of freedom is thus the total number of primary sampling units (54) minus the number of strata (27).

[b] An asterisk indicates statistical significance at the .05 level.

[c] Values of R^2 were determined from the weighted least squares solution; two values are given. The uppermost value is for the model presented; the value in parenthesis is for the model resulting after removing all nonsignificant predictors from the equation.

Table G.18

ANALYSIS SUMMARY OF TESTS FOR REDUCTION OF MAIN EFFECTS MODEL: EDUCATIONAL EXPECTATIONS

Factor Tested	Degrees of Freedom for Model	Degrees of Freedom for Error[a]	F[b]	R^2[c]
Total Model	11	27	13.22*	.164 (.163)
Age	1	27	26.48*	--
Extent of UB Participation	3	27	15.13*	--
Race	2	27	8.06*	--
Grade	2	27	9.29*	--
Poverty Status	1	27	5.70*	--
Academic Risk	1	27	28.63*	--
Sex	1	27	0.15	--

NOTE: The tests reported are based on weighted least squares analyses of the 2994 cases for whom no indeterminacy existed for predictor variables or for educational expectations. Actual tests are based on a Taylorized deviation approach, as defined in Section IV of this Appendix.

[a] The Taylorized deviation approach determines the appropriate estimate of the error term within sampling strata; the associated degrees of freedom is thus the total number of primary sampling units (54) minus the number of strata (27).

[b] An asterisk indicates statistical significance at the .05 level.

[c] Values of R^2 were determined from the weighted least squares solution; two values are given. The uppermost value is for the model presented; the value in parenthesis is for the model resulting after removing all nonsignificant predictors from the equation.

Table G.19

REGRESSION PARAMETERS AND ASSOCIATED STANDARD ERRORS FOR THE REDUCED MODEL PREDICTING HIGHEST GRADE COMPLETED

Predictor Variable[a]	Raw Regression Weight[b]	Standard Error[c]
Intercept[d]	12.701	0.093
1-year UB participation	0.071	0.106
2-years UB participation	0.349	0.116
3-years UB participation	0.400	0.097
Black	0.144	0.112
Other ethnicity[e]	- 0.174	0.110
Age[f]	- 0.266	0.060
10th grade cohort	- 0.378	0.102
12th grade cohort	0.820	0.157
Academic risk status[g]	- 0.422	0.099

NOTE: The regression analysis reported in this table was computed using weighted data and the weighted least squares criterion for the 3192 cases in which no data indeterminacy existed for either the full predictor variable set or for highest grade completed.

[a] With the exception of age, the predictor variables are dummy variables reflecting group membership as indicated in the row label or in a footnote.

[b] These are weights which may be applied directly to the value of the variable under consideration.

[c] Computed using the Taylorized deviations approach defined in Section IV of this appendix.

[d] The predictor variables were coded in such a way that this parameter represents the predicted value of highest grade completed for a white, non-risk nonparticipant who was 16 years old and in the eleventh grade during the 1973-74 school year.

[e] Other than Black or white.

[f] Age reflects age in the spring of 1973 and has been recoded by subtraction of 16.

[g] The category "academic risk" is the specific group coded.

Table G.20

REGRESSION PARAMETERS AND ASSOCIATED STANDARD ERRORS FOR THE REDUCED MODEL PREDICTING POSTSECONDARY ENTRY

Predictor Variable[a]	Raw Regression Weight[b]	Standard Error[c]
Intercept[d]	0.631	0.051
1-year UB participation	0.047	0.051
2-years UB participation	0.177	0.047
3-years UB participation	0.255	0.039
Black	0.014	0.052
Other ethnicity[e]	-0.153	0.053
Sex[f]	0.112	0.049
Age[g]	-0.088	0.021
10th grade cohort	-0.084	0.061
12th grade cohort	0.136	0.058
Academic risk status[h]	-0.212	0.038

NOTE: The regression analysis reported in this table was computed using weighted data and the weighted least squares criterion for the 3203 cases in which no data indeterminacy existed for either the full predictor variable set or for postsecondary entry.

[a] With the exception of age, the predictor variables are dummy variables reflecting group membership as indicated in the row label or in a footnote.

[b] These are weights which may be applied directly to the value of the variable under consideration.

[c] Computed using the Taylorized deviations approach defined in Section IV of this appendix.

[d] The predictor variables were coded in such a way that this parameter represents the likelihood of postsecondary entry for a white, non-risk, male nonparticipant who was 16 years old and in the eleventh grade during the 1973-74 school year.

[e] Other than Black or white.

[f] The sex category "female" is the specific group coded.

[g] Age reflects age in spring of 1973 and has been recoded by subtraction of 16.

[h] The category "academic risk" is the specific group coded.

Table G.21

REGRESSION PARAMETERS AND ASSOCIATED STANDARD ERRORS FOR THE REDUCED MODEL PREDICTING EDUCATIONAL EXPECTATIONS

Predictor Variable[a]	Raw Regression Weight[b]	Standard Error[c]
Intercept[d]	3.373	0.136
1-year UB participation	0.327	0.137
2-years UB participation	0.631	0.120
3-years UB participation	0.806	0.120
Black	0.484	0.175
Other ethnicity[e]	-0.217	0.251
Age[f]	-0.370	0.071
10th grade cohort	-0.358	0.169
12th grade cohort	0.510	0.151
Poverty status[g]	-0.377	0.160
Academic risk status[h]	-0.559	0.103

NOTE: The regression analysis reported in this table was computed using weighted data and the weighted least squares criterion for the 2994 cases in which no data indeterminacy existed for either the full predictor variable set or for educational expectations.

[a] With the exception of age, the predictor variables are dummy variables reflecting group membership as indicated in the row label or in a footnote.

[b] These are weights which may be applied directly to the value of the variable under consideration.

[c] Computed using the Taylorized deviations approach defined in Section IV of this appendix.

[d] The predictor variables were coded in such a way that this parameter represents the likelihood of educational expectations for a white, non-risk, nonparticipant who was not within the poverty level and was 16 years old and in the eleventh grade during the 1973-74 school year.

[e] Other than Black or white.

[f] Age reflects age in spring of 1973 and has been recoded by subtraction of 16.

[g] The category "within poverty status" is the specific group coded.

[h] The category "academic risk" is the specific group coded.